# CARNEGIE INSTITUTION

OF

# WASHINGTON

# YEAR BOOK

No. 3

1904


PUBLISHED BY THE INSTITUTION
WASHINGTON, U. S. A.
JANUARY, 1905

PRESS OF JUDD & DETWEILER
WASHINGTON, D. C.

# OFFICERS FOR THE YEAR 1905

*President of the Institution*

ROBERT S. WOODWARD



*Executive Committee*

CARROLL D. WRIGHT, *Chairman*
*CHARLES D. WALCOTT, *Secretary*

| | | |
|---|---|---|
| JOHN S. BILLINGS | JOHN HAY | ELIHU ROOT |
| DANIEL C. GILMAN | S. WEIR MITCHELL | *ROBERT S. WOODWARD |

*Finance Committee*

| | | |
|---|---|---|
| LYMAN J. GAGE | HENRY L. HIGGINSON | D. O. MILLS |

* Ex-officio member

# CONTENTS.

# LIST OF ACCOMPANYING PAPERS.



---

# ILLUSTRATIONS.

## PLATES.



## TEXT FIGURES.

## ARTICLES OF INCORPORATION.

The Carnegie Institution was originally organized under the law governing the organization of corporations in the District of Columbia. Owing to certain limitations in the law, the Trustees deemed it desirable to obtain articles of incorporation from the Congress. Accordingly, articles of incorporation were prepared, submitted to the Congress, amended by the Congress, and enacted into statute by the Congress and the signature of the President.

Organization under the new articles of incorporation was effected on May 18, 1904. Resolutions were passed electing the same Executive Committee and officers as those of the Carnegie Institution organized in 1902 and continuing all instructions and authorizations given to the Executive Committee by the old organization.

PUBLIC No. 260.—An Act To incorporate the Carnegie Institution of Washington.

*Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled*, That the persons following, being persons who are now trustees of the Carnegie Institution, namely, Alexander Agassiz, John S. Billings, John L. Cadwalader, Cleveland H. Dodge, William N. Frew, Lyman J. Gage, Daniel C. Gilman, John Hay, Henry L. Higginson, William Wirt Howe, Charles L. Hutchinson, Samuel P. Langley, William Lindsay, Seth Low, Wayne MacVeagh, Darius O. Mills, S. Weir Mitchell, William W. Morrow, Ethan A. Hitchcock, Elihu Root, John C. Spooner, Andrew D. White, Charles D. Walcott, Carroll D. Wright, their associates and successors, duly chosen, are hereby incorporated and declared to be a body corporate by the name of the Carnegie Institution of Washington and by that name shall be known and have perpetual succession, with the powers, limitations, and restrictions herein contained.

SEC. 2. That the objects of the corporation shall be to encourage, in the broadest and most liberal manner, investigation, research, and discovery, and the application of knowledge to the improvement of mankind; and in particular—

(a) To conduct, endow, and assist investigation in any department of science, literature, or art, and to this end to cooperate with governments, universities, colleges, technical schools, learned societies, and individuals.

(b) To appoint committees of experts to direct special lines of research.

(c) To publish and distribute documents.

(d) To conduct lectures, hold meetings, and acquire and maintain a library.

(e) To purchase such property, real or personal, and construct such building or buildings as may be necessary to carry on the work of the corporation.

(f) In general, to do and perform all things necessary to promote the objects of the institution, with full power, however, to the trustees hereinafter appointed and their successors from time to time to modify the conditions and regulations under which the work shall be carried on, so as to secure the application of the funds in the manner best adapted to the conditions of the time, provided that the objects of the corporation shall at all times be among the foregoing or kindred thereto.

SEC. 3. That the direction and management of the affairs of the corporation and the control and disposal of its property and funds shall be vested in a board of trustees, twenty-two in number, to be composed of the following individuals: Alexander Agassiz, John S. Billings, John L. Cadwalader, Cleveland H. Dodge, William N. Frew, Lyman J. Gage, Daniel C. Gilman, John Hay, Henry L. Higginson, William Wirt Howe, Charles L. Hutchinson, Samuel P. Langley, William Lindsay, Seth Low, Wayne MacVeagh, Darius O. Mills, S. Weir Mitchell, William W. Morrow, Ethan A. Hitchcock, Elihu Root, John C. Spooner, Andrew D. White, Charles D. Walcott, Carroll D. Wright, who shall constitute the first board of trustees. The board of trustees shall have power from time to time to increase its membership to not more than twenty-seven members. Vacancies occasioned by death, resignation, or otherwise shall be filled by the remaining trustees in such manner as the by-laws shall prescribe; and the persons so elected shall thereupon become trustees and also members of the said corporation. The principal place of business of the said corporation shall be the city of Washington, in the District of Columbia.

SEC. 4. That such board of trustees shall be entitled to take, hold and administer the securities, funds, and property so transferred by said Andrew Carnegie to the trustees of the Carnegie Institution and such other funds or property as may at any time be given, devised, or bequeathed to them, or to such corporation, for the purposes of the trust; and with full power from time to time to adopt a common

seal, to appoint such officers, members of the board of trustees or otherwise, and such employees as may be deemed necessary in carrying on the business of the corporation, at such salaries or with such remuneration as they may deem proper; and with full power to adopt by-laws from time to time and such rules or regulations as may be necessary to secure the safe and convenient transaction of the business of the corporation; and with full power and discretion to deal with and expend the income of the corporation in such manner as in their judgment will best promote the objects herein set forth and in general to have and use all powers and authority necessary to promote such objects and carry out the purposes of the donor. The said trustees shall have further power from time to time to hold as investments the securities hereinabove referred to so transferred by Andrew Carnegie, and any property which has been or may be transferred to them or such corporation by Andrew Carnegie or by any other person, persons, or corporation, and to invest any sums or amounts from time to time in such securities and in such form and manner as are permitted to trustees or to charitable or literary corporations for investment, according to the laws of the States of New York, Pennsylvania, or Massachusetts, or in such securities as are authorized for investment by the said deed of trust so executed by Andrew Carnegie, or by any deed of gift or last will and testament to be hereafter made or executed.

SEC. 5. That the said corporation may take and hold any additional donations, grants, devises, or bequests which may be made in further support of the purposes of the said corporation, and may include in the expenses thereof the personal expenses which the trustees may incur in attending meetings or otherwise in carrying out the business of the trust, but the services of the trustees as such shall be gratuitous.

SEC. 6. That as soon as may be possible after the passage of this Act a meeting of the trustees hereinbefore named shall be called by Daniel C. Gilman, John S. Billings, Charles D. Walcott, S. Weir Mitchell, John Hay, Elihu Root, and Carroll D. Wright, or any four of them, at the city of Washington, in the District of Columbia, by notice served in person or by mail addressed to each trustee at his place of residence; and the said trustees, or a majority thereof, being assembled, shall organize and proceed to adopt by-laws, to elect officers and appoint committees, and generally to organize the said corporation; and said trustees herein named, on behalf of the corporation hereby incorporated, shall thereupon receive, take over,

and enter into possession, custody, and management of all property, real or personal, of the corporation heretofore known as the Carnegie Institution, incorporated, as hereinbefore set forth under "An Act to establish a Code of Law for the District of Columbia, January fourth, nineteen hundred and two," and to all its rights, contracts, claims, and property of any kind or nature; and the several officers of such corporation, or any other person having charge of any of the securities, funds, real or personal, books or property thereof, shall, on demand, deliver the same to the said trustees appointed by this Act or to the persons appointed by them to receive the same; and the trustees of the existing corporation and the trustees herein named shall and may take such other steps as shall be necessary to carry out the purposes of this Act.

SEC. 7. That the rights of the creditors of the said existing corporation known as the Carnegie Institution shall not in any manner be impaired by the passage of this Act, or the transfer of the property hereinbefore mentioned, nor shall any liability or obligation for the payment of any sums due or to become due, or any claim or demand, in any manner or for any cause existing against the said existing corporation, be released or impaired; but such corporation hereby incorporated is declared to succeed to the obligations and liabilities and to be held liable to pay and discharge all of the debts, liabilities, and contracts of the said corporation so existing to the same effect as if such new corporation had itself incurred the obligation or liability to pay such debt or damages, and no such action or proceeding before any court or tribunal shall be deemed to have abated or been discontinued by reason of the passage of this Act.

SEC. 8. That Congress may from time to time alter, repeal, or modify this Act of incorporation, but no contract or individual right made or acquired shall thereby be divested or impaired.

SEC. 9. That this Act shall take effect immediately.

Approved, April 28, 1904.

## BY-LAWS OF THE INSTITUTION.

Adopted December 13, 1904.

### ARTICLE I.

#### THE TRUSTEES.

1. The Board of Trustees shall consist of twenty-four members, with power to increase its membership to not more than twenty-seven members. The Trustees shall hold office continuously and not for a stated term.

2. In case any Trustee shall fail to attend three successive annual meetings of the Board he shall thereupon cease to be a Trustee.

3. No Trustee shall receive any compensation for his services as such.

4. All vacancies in the Board of Trustees shall be filled by the Trustees by ballot. No person shall be elected, however, who shall not have been nominated at a preceding annual or special meeting, except by the unanimous consent of the members present at a meeting.

### ARTICLE II.

#### MEETINGS.

1. The annual meeting of the Board of Trustees shall be held in the City of Washington, in the District of Columbia, on the second Tuesday of December in each year.

2. Special meetings of the Board may be called by the Executive Committee by notice served personally upon, or mailed to the usual address of, each Trustee twenty days prior to the meeting.

3. Special meetings shall, moreover, be called in the same manner by the Chairman upon the written request of seven members of the Board.

### ARTICLE III.

#### OFFICERS OF THE BOARD.

1. The officers of the Board shall be a Chairman of the Board, a Vice-Chairman, and a Secretary, who shall be elected by the Trustees, from the members of the Board, by ballot to serve for a term of three years. All vacancies shall be filled by the Board for the unexpired term; provided, however, that the Executive Committee shall have power to fill a vacancy in the office of Secretary to serve until the next meeting of the Board of Trustees.

2. The Chairman shall preside at all meetings and shall have the usual powers of a presiding officer.

3. The Vice-Chairman, in the absence or disability of the Chairman, shall perform his duties.

4. The Secretary shall issue notices of meetings of the Board, record its transactions, and conduct that part of the correspondence relating to the Board and to his duties. He shall execute all deeds, contracts or other instruments on behalf of the corporation, when duly authorized. He shall have custody of the seal of the corporation and shall affix the same whenever authorized to do so by the Board of Trustees or by the Executive Committee or the Finance Committee.

### ARTICLE IV.

#### EXECUTIVE ADMINISTRATION.

*The President.*

1. There shall be a President who shall be elected by ballot by, and hold office during the pleasure of, the Board, who shall be the chief executive officer of the Institution. The President, subject to the control of the Board and the Executive Committee, shall have general charge of all matters of administration and supervision of all arrangements for research and other work undertaken by the Institution or with its funds. He shall devote his entire time to the affairs of the Institution. He shall prepare and submit to the Board of Trustees and to the Executive Committee plans and suggestions for the work of the Institution, shall conduct its general correspondence and the correspondence with applicants for grants and with the special advisers of the Committee, and shall present his recommendations in each case to the Executive Committee for decision. All proposals and requests for grants shall be referred to the President for consideration and report. He shall have power to remove and appoint subordinate employees and shall be *ex officio* a member of the Executive Committee.

2. He shall be the legal custodian of all property of the Institution whose custody is not otherwise provided for. He shall be responsible for the expenditure and disbursement of all funds of the Institution in accordance with the directions of the Board and of the Executive Committee, and shall keep accurate accounts of all receipts and disbursements. He shall submit to the Board of Trustees at least one month before its annual meeting in December a written report of the operations and business of the Institution for the preceding fiscal year with his recommendations for work and appropriations for the succeeding fiscal year, which shall be forthwith transmitted to each member of the Board.

3. He shall attend all meetings of the Board of Trustees.

## Article V.

### COMMITTEES.

1. There shall be the following standing Committees, viz, an Executive Committee and a Finance Committee.

2. The Executive Committee shall consist of the Chairman and Secretary of the Board of Trustees and the President of the Institution *ex officio* and, in addition, five trustees to be elected by the Board by ballot for a term of three years, who shall be eligible for re-election. Any member elected to fill a vacancy shall serve for the remainder of his predecessor's term: Provided, however, that of the Executive Committee first elected after the adoption of these by-laws two shall serve for one year, two shall serve for two years, and one shall serve for three years; and such Committee shall determine their respective terms by lot.

3. The Executive Committee shall, when the Board is not in session and has not given specific directions, have general control of the administration of the affairs of the corporation and general supervision of all arrangements for administration, research, and other matters undertaken or promoted by the Institution; shall appoint advisory committees for specific duties; shall determine all payments and salaries; and keep a written record of all transactions and expenditures and submit the same to the Board of Trustees at each meeting, and it shall also submit to the Board of Trustees a printed or typewritten report of each of its meetings, and at the annual meeting shall submit to the Board a report for publication.

4. The Executive Committee shall have general charge and control of all appropriations made by the Board.

5. The Finance Committee shall consist of three members to be elected by the Board of Trustees by ballot for a term of three years.

6. The Finance Committee shall have general charge of the investments and funds of the corporation, and shall care for and dispose of the same subject to the directions of the Board and of the Executive Committee. It shall consider and recommend to the Board of Trustees such measures as in its opinion will promote the financial interests of the Institution, and shall make a report at each meeting of the Board.

7. All vacancies occurring in the Executive Committee and the Finance Committee shall be filled by the Trustees at the next regular meeting.

8. The terms of all officers and of all members of committees shall continue until their successors are elected or appointed.

## ARTICLE VI.

### FINANCIAL ADMINISTRATION.

1. No expenditure shall be authorized or made except in pursuance of a previous appropriation by the Board of Trustees.

2. The fiscal year of the Institution shall commence on the first day of November in each year.

3. The Executive Committee, at least one month prior to the annual meeting in each year, shall cause the accounts of the Institution to be audited by a skilled accountant, to be appointed by the Chairman of the Board, and shall submit to the annual meeting of the Board a full statement of the finances and work of the Institution and a detailed estimate of the expenditures for the succeeding year.

4. The Board of Trustees, at the annual meeting in each year, shall make general appropriations for the ensuing fiscal year; but nothing contained herein shall prevent the Board of Trustees from making special appropriations at any meeting.

5. The securities of the Institution and evidences of property shall be deposited in such safe deposit or other corporation and under such safeguards as the Trustees and Executive Committee shall designate; and the moneys of the Institution shall be deposited in such banks or depositories as may from time to time be designated by the Executive Committee.

## ARTICLE VII.

### AMENDMENT OF BY-LAWS.

1. These by-laws may be amended at any annual or special meeting of the Board of Trustees by a two-thirds vote of the members present, provided written notice of the proposed amendment shall have been served personally upon, or mailed to the usual address of, each member of the Board twenty days prior to the meeting.

## MINUTES OF SECOND MEETING OF THE BOARD OF TRUSTEES.

[Abstract.]

The meeting was held in Washington, at the New Willard Hotel, on Tuesday, December 13, 1904, at 10 o'clock a. m. At 12.55 a recess was taken until 2 p. m.

The Chairman, Mr. Billings, occupied the chair.

The Secretary called the roll, and the following Trustees responded: Messrs. Billings, Cadwalader, Dodge, Frew, Gilman, Hay, Higginson, Hitchcock, Hutchinson, Langley, Lindsay, Low, MacVeagh, Mills, Mitchell, Morrow, Root, Walcott, White, and Wright.

Absent: Messrs. Agassiz, Howe, Gage, and Spooner.

Letters were received from Messrs. Agassiz, Gage, and Howe regretting their inability to be present.

The minutes of the last meeting of the Board were presented, and on motion full reading was dispensed with and they were approved as per abstract furnished each member.

The President presented his resignation, as follows:

CARNEGIE INSTITUTION OF WASHINGTON,
*December 13, 1904.*

*To the Trustees of the Carnegie Institution.*

GENTLEMEN: At your meeting on December 8, 1903, I presented a letter saying:

"When I had the honor of being chosen the first President of the Carnegie Institution, I said to the Trustees that from the nature of the case my tenure of office must be short, for, having passed the age of seventy years, I was looking forward to a release from serious official responsibilities. The term of five years was fixed in the by-laws, and three of them will have passed at the next annual meeting of the Board. It is my intention at that time to resign the office of President, and this early notice is given in order that the Trustees may be prepared then to take such action as may seem to them wise."

In accordance with this intimation, I now resign the office of President of the Carnegie Institution, and, as the title of the chief executive may perhaps be changed, I will add that I am not a candidate for reappointment under any other designation.

In taking this step, I beg leave to assure the Board of my continued interest in the development of this Institution according to the purposes of the founder; and I express to the members of the Board, collectively and individually, my highest respect.

It has been an honor and a privilege to be so closely associated as I have been with the organization and progress of an institution which bids fair to be a most potent factor in the advancement of knowledge and in the encouragement of scientific men.

I am, gentlemen, very respectfully yours,

DANIEL C. GILMAN.

The following motion was then offered and passed:

*Resolved*, That the resignation of President Gilman be accepted; and in thus severing the harmonious relations which have existed between the President and the Board and the President and the Executive Committee the Trustees desire to express their full appreciation of the prestige that the retiring officer has brought to the Carnegie Institution of Washington by his presidency.

The Secretary referred to various details of business and submitted the cash statement and financial statement as shown on pages 19 and 20.

The Secretary also reported that since October 31, 1904, he had collected on sales of publications $589.01, and expended $31,895.21, leaving a cash balance on hand of $438,654.97 to date.

The consideration of the by-laws was next taken up. The by-laws as amended and adopted are printed on pages 13–16.

After discussion and various suggestions as to the qualifications needed by a president of the Carnegie Institution of Washington, a ballot resulted in the election of Dr. Robert S. Woodward, Dean of the Scientific Faculty of Columbia University, New York.

The election of members of the Executive Committee to fill the vacancies caused by the expiration of the terms of Messrs. Billings and Walcott resulted in their re-election to the class of 1907.

On submission of the report of the Executive Committee the Chairman and Secretary made a general statement of the plan of work and financial outlook. After discussion and some minor changes, resolutions were passed making the following general appropriations:

| | |
|---|---|
| Reserve fund | $50,000 |
| Publication fund, to be continuously available | 40,000 |
| Administration | 50,000 |
| Grants for departments and large projects | 310,000 |
| Grants for miscellaneous researches | 168,000 |

At 4.50 p. m. the Board adjourned.

*Cash Statement at the Close of the Fiscal Year ending October 31, 1904.*

| Receipts. | | | Amount. |
|---|---|---|---|
| 1903. | | | |
| October 31. Balance. . . . . . . . . . . . . . . . . . . . . . . . . . . . | | | $445,471 69 |
| Interest: | | | |
| U.S. Steel Corporation. | $500,000 | | |
| Atchison, Topeka and S. Fe Ry. Co. . . . . . . | 5,000 | | |
| N. Pacific Ry. Co. . . . . | 4,500 | | |
| N. Pacific-Gt. Northern | 2,500 | | |
| Lake Shore and Michigan Southern . . . . . . | 2,000 | | |
| Deposit U. S. Trust Co. | 18,926 87 | | |
| Deposit Am. Sec. and Trust Co. . . . . . . . . . . | 77 39 | $533,004 26 | |
| Sales of publications: | | | |
| Index Medicus. . . . . . . | 2,370 47 | | |
| Year Book. . . . . . . . | 52 85 | | |
| Other publications. . . . | 12 75 | 2,436 07 | |
| Revertments. . . . . . . . . . . . . . . . . . . . . . . | | 329 33 | |
| Marine Biol. Lab., Tortugas, Fla.: | | | |
| A. G. Mayer. . . . . . . . . . . . . . . . . . . | | 669 70 | 536,439 36 |
| | | | 981,911 05 |

| Disbursements. | | Amount. |
|---|---|---|
| Investments: | | |
| $50,000 N. Pacific-Gt. Northern 4 % Joint Bonds. . . . . . . . . . | $46,500 | |
| $50,000 N. Pacific Ry. Co. Prior L. Ry. and L. Grant 4's. . . . . | 51,312 50 | |
| $50,000 Atchison, Topeka and S. Fe Ry. Co. G. Mtg. 4's. . . . | 50,125 | |
| $50,000 Lake Shore and Mich. Southern 4 % D. Bonds. . . . . . | 48,222 22 | $196,159 72 |
| Large grants. . . . . . . . . . . . . . . . . . . . | 49,848 46 | |
| Minor grants. . . . . . . . . . . . . . . . . . . . . | 187,634 53 | |
| Special grants. . . . . . . . . . . . . . . . . . . | 29,749 20 | 267,232 19 |
| Publication. . . . . . . . . . . . . . . . . . . . . . . . . . . . . . | | 11,590 82 |
| Administration. . . . . . . . . . . . . . . . . . . . . . . . . . . . | | 36,967 15 |
| | | 511,949 88 |
| Balance: U. S. Trust Co., N. Y. | 461,902 46 | |
| Balance: Nat'l City Bank, N. Y. | 7,746 67 | |
| Balance: Am. Sec. and Trust Co. | 312 04 | 469,961 17 |
| | | 981,911 05 |

CHAS. D. WALCOTT,
*Secretary.*

*Financial Statement.*

| | Dr. | Cr. |
|---|---|---|
| Endowment | | $10,000,000 |
| Reserve Fund | | 200,000 |
| Investments: | | |
| U. S. Steel Corporation Bonds, 5% | $10,000,000 | |
| $100,000 Atch., Topeka and Santa Fe Ry. Co. Gen'l Mtg. 4% 100-year Gold Bonds, Oct. 1, 1995 | 100,112 50 | |
| $100,000 N. Pac. Ry. Co. Prior Lien Ry. and Land Grant Gold Bonds, Jan. 1, 1997, 4% | 101,800 | |
| $50,000 Northern Pacific-Great Northern 4% Joint Bonds, Chicago, Burlington and Q. collateral, July 1, 1921 | 46,500 | |
| $50,000 Lake Shore and Mich. Southern 4% D. Bonds | 48,222 22 | |
| Interest: Reserve fund investment | | 10,000 |
| Other investments | | 380 69 |
| Sales of publications | | 102 03 |
| Grants: Large | | 69,321 24 |
| Special | | 13,250 80 |
| Minor | | 77,174 13 |
| Publication | | 67,470 65 |
| Administration | | 25,630 08 |
| Furniture | | 1,065 51 |
| Seal | | 1,500 |
| Cash | 469,961 17 | |
| Available fund | | 300,700 76 |
| | $10,766,595 89 | $10,766,595 89 |

# REPORT OF EXECUTIVE COMMITTEE ON THE WORK OF THE YEAR.

The Executive Committee began consideration of the various directions and authorizations given by the Board of Trustees immediately after the adjournment of the Board, December 8, 1903; also of matters recommended by the committee and approved by the Board.

The work of the committee and its recommendations for the fiscal year 1904–1905 are shown in this report.

During the fiscal year the committee held eight meetings. Its organization continued the same as for the fiscal year 1902–1903. Mr. Gilman acted as chairman and Mr. Walcott as secretary.

### APPROPRIATIONS.

At the annual meeting of the Board, December 8, 1903, the following appropriations were made for large projects:

| | | |
|---|---|---|
| Tropical Pacific exploration | | *$40,000 |
| Department of Experimental Biology | | 70,000 |
| Department of Terrestrial Magnetism | | 20,000 |
| Trans-Caspian Expedition, archeological exploration | | 18,000 |
| Geophysical research | | 25,000 |
| Investigation of mineral fusion and solution under pressure | $12,500 | |
| Study of elasticity and plasticity of solid bodies upon finite deformation | 7,500 | |
| Preparation of a bibliography of geophysics | 5,000 | |
| Department of Economics and Sociology | | 30,000 |
| Bureau of Historical Research | | 8,500 |
| | | $211,500 |

* It being impracticable to secure the services of the person whom the Executive Committee expected to take charge of this work, the project was abandoned and the appropriation not drawn upon.

# REPORTS ON LARGE PROJECTS.

## DEPARTMENT OF EXPERIMENTAL BIOLOGY.

The subject of research in zoölogy was before the Executive Committee at its earliest meetings, and was under consideration for nearly two years before the specific recommendations for any large projects directly in charge of the Carnegie Institution were presented to the Board of Trustees. In Year Book No. 1 the special advisory committee on zoölogy made several recommendations of a broad bearing, one of them being that of establishing a permanent biological laboratory as a central station for marine biology in general. In the same Year Book there were printed two schemes for the establishment of biological experiment stations for the study of evolution—one by Dr. C. B. Davenport, who favored Cold Spring Harbor, Long Island, and a second by Prof. Roswell P. Johnson, who favored a protected marine shore near fresh-water ponds. The Executive Committee consulted with many experts and carefully investigated the feasibility of making the Marine Biological Laboratory, at Woods Hole, Mass., a central station. This was found to be impracticable, and the Executive Committee stated in its report to the Board of Trustees for 1903 that it had concluded that the best mode of dealing with this important field of research was to organize a Biological Experimental Department, to which could be referred all questions and problems of evolution, specific differentiation, heredity, etc. This was to include the establishment of an investigating station at Cold Spring Harbor, where ground and some buildings were offered, and also the establishment of a collection and experimental marine biological station at the Dry Tortugas.

The above conclusions were accompanied by a recommendation that the department be established and allotments made to begin the work. The Board of Trustees approved the recommendations.

The Department of Experimental Biology was organized by the appointment of Dr. Charles B. Davenport as Director of the Station for Experimental Evolution at Cold Spring Harbor, Long Island, and Dr. Alfred G. Mayer as Director of the Marine Biological Laboratory at the Dry Tortugas, Florida.

A grant of $34,250 was made to the station at Cold Spring Harbor, and of $20,000 to the Marine Biological Laboratory at the Dry Tortugas.

The reports of the directors follow.

## First Report of Station for Experimental Evolution under Department of Experimental Biology.

By C. B. Davenport.

At the request of the Executive Committee of the Carnegie Institution, I submitted a plan of organization of the department in December, 1903, and, in detail, of the Station for Experimental Evolution.

It was decided to locate the station at Cold Spring Harbor, Long Island. The superior advantages of other localities were fully considered. California offers a more equable climate, where outdoor work could be pursued throughout the year; the proximity of lofty mountains would be advantageous. Two important considerations favored the selection of Long Island: First, its accessibility to the greater number of workers in this field, and, second, its proximity to extensive libraries, making the upbuilding of a large library at the station unnecessary. The points of fitness of Cold Spring Harbor for the proposed work, besides those of central location and proximity to great libraries, are as follows: The free offer of about ten acres of land, with house and stable and horse shed; the situation of this land on the sea, with wharf, and on a fresh-water creek with a permanent stream running across the land, and with elevations varying from sea-level to 50 feet above sea-level; the location is among interesting and intelligent neighbors, with the desire and the means of helping the work proposed; the surrounding country is well watered, densely forested, and hilly, offering a great variety of habitats, whose fauna and flora have long been thoroughly studied. The offer of this advantageous property was made by the Wawepex Society, which holds it in trust from the late John D. Jones.

The writer spent the winter months in New York in arranging for the transfer of the property, in visiting the architects, and in purchasing supplies for the new station. Early in February a caretaker, Mr. John N. Johnson, took up his residence at Cold Spring Harbor, and work with living animals there has been carried out continuously since. On May 1 Dr. Shull, Miss Lutz, and Mr. T. E. Kelly began resident work, and on June 1 Mr. Frank E. Lutz arrived.

On Saturday, June 11, the formal opening of the station was celebrated by exercises. Through the courtesy of the Long Island R. R. Co. a special car brought some fifty guests from New York, and an equal number attended from the neighborhood. After luncheon at the director's residence the following addresses (for full report see pp. 33-49) were given in the Biological Laboratory, whose grounds adjoin those of the station:

PROGRAM.

1. Introductory address by the director of the station.
2. Presentation address, by Mr. W. R. T. Jones, governor of the Wawepex Society.
3. Response by Dr. John S. Billings, chairman of the Board of Trustees, Carnegie Institution.
4. Address of welcome to the station on behalf of the Brooklyn Institute by Prof. Franklin W. Hooper, director of the Brooklyn Institute of Arts and Sciences.
5. Scientific address, The aims of experimental evolution, by Prof. Hugo de Vries, professor of botany at the University of Amsterdam and director of its botanic garden.

DESCRIPTION OF GROUNDS AND BUILDINGS.

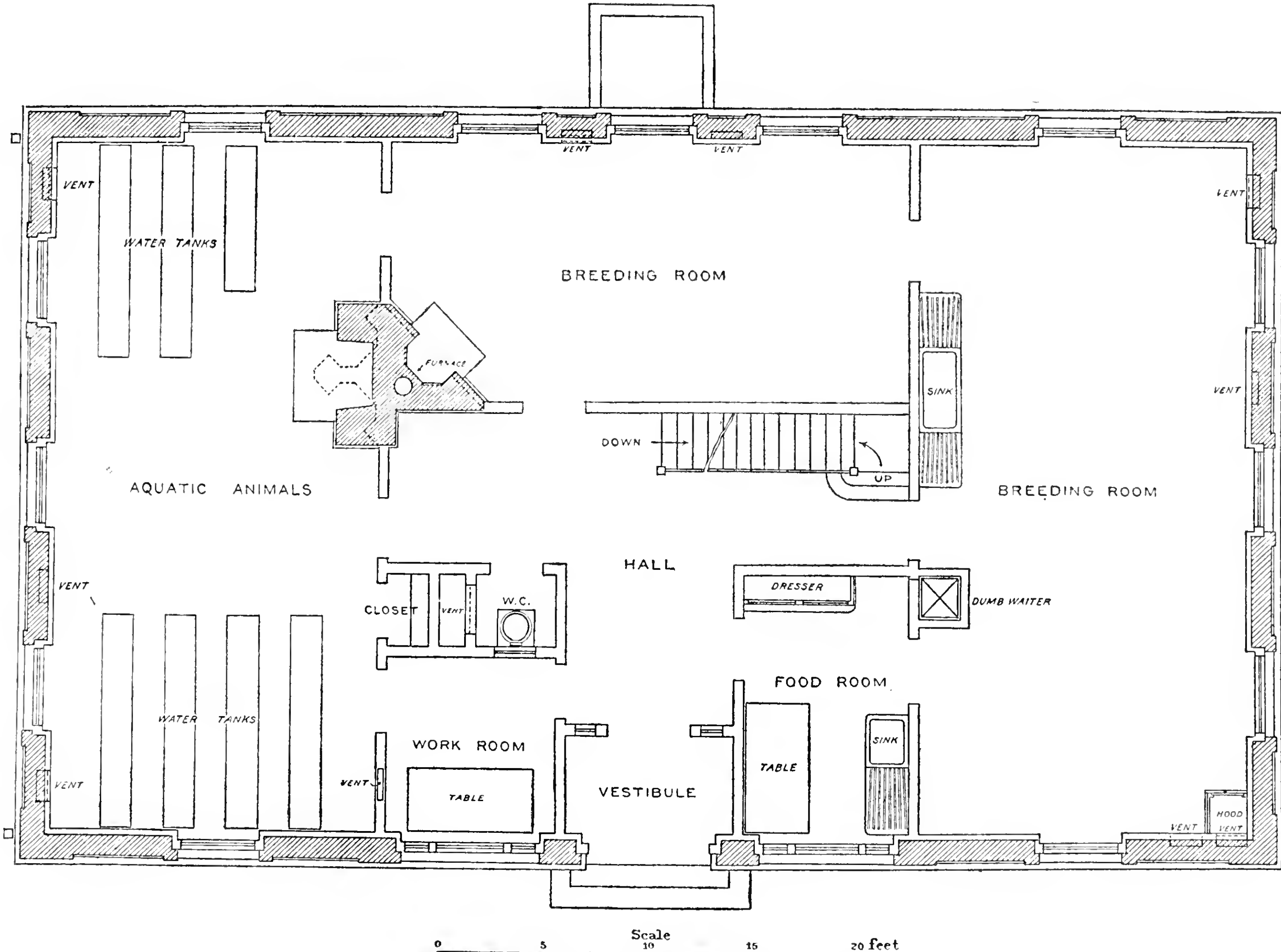
The land, leased for fifty years to the Carnegie Institution of Washington for a nominal sum, is situated, as shown on the map on page 25, at the head of Cold Spring Harbor, about 34 miles from Long Island City by road and rail and 14 miles in a direct line from the boundary of Greater New York.

The property is bounded on the northeast by the harbor, on the east by the Natchaquatuck creek, on the south by the public highway, which separates it from the grounds of the New York State fish hatchery, on the west by private grounds and a private road, and on the northwest by the lands of the Wawepex Society leased to the Brooklyn Institute. The whole lot of land is divided into a smaller and a larger part by a private road. On this piece of land is a large house on the site of the old homestead of John D. Jones and his brothers and sisters, some of whom are still living on Long Island. This house will be used as the director's residence. Something over an acre is reserved as the house plot. Most of the rest of the main plot of some five or six acres is surrounded by a wire fence (77 inches high and supported on iron posts) for the better protection of live-stock and the experimental garden.

On the wharf there stands a shed, very useful for the temporary shelter of lumber, coal, etc., brought to the station by boat. Just east is a large salt-water fish-pond, and beyond is a small boat and bath house, near which ways will lead to a larger boat-house for the protection of the station launch during the winter. Near this boat-house and inside the main inclosure is a driven well 204 feet deep, flowing 9 gallons per minute. This will supply the residence, stable, and laboratory, by means of an electric pump with a capacity of 15 gallons per minute. It is proposed to supply the tanks in the cellar and first floor of the laboratory from a spring in the ravine.

The laboratory building, which is being erected under the superintendence of Messrs. Kirby, Petit & Green, of New York city, by

PLATE 1.




COLD SPRING HARBOR STATION, FIRST FLOOR PLAN.

Messrs. Rogers & Blydenburgh, general contractors, of Babylon, Long Island, is 60 feet by 35 feet, and consists of two stories, cellar, and attic. It is built of brick in Italian Renaissance style, with framed partitions and floors above the cellar. The roof is of tin. Iron lath is used and the floors are covered with asbestolith, so that the building is semi-fireproof. The front elevation is shown on page 26 and three floor plans on plates 1, 2, and 3.

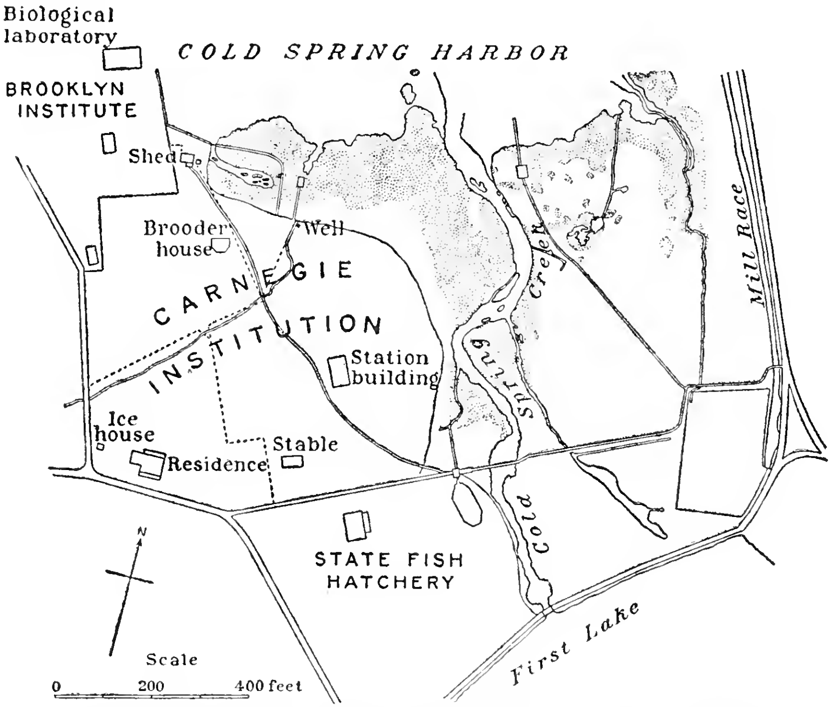



FIG. 1.—Plan showing main plot of ground, buildings, etc., Cold Spring Harbor Station.

In the cellar, which has windows on the east half, are a work-room, a coal-room, and a room for the storage of food and agricultural implements. In the unilluminated half are a photographic room, a refrigerator room, and one for cave studies on both terrestrial and aquatic organisms. On the ground floor are two large rooms for breeding terrestrial animals, one for aquatic animals, one for preparing food, and one small work-room. On the second floor there are five research rooms, a secretary's room, a small library with a capacity of 1,000 books, and a large glass-covered room for breeding plants and animals. In the attic is a single room 39 feet by 14 feet and 6

feet high under the eaves, rising to 8 feet. This room, lighted by ten small windows, has a capacity of about 7,000 books.

Every one of the work-rooms of the building is supplied with salt water and both cold and hot fresh water; each has electricity as the main source of light and power, and is piped for acetylene gas. There is an intercommunicating telephone system, and additional wire, sufficient to connect the different parts of the property, has been placed. A dumb-waiter places the main breeding-rooms in connection with the food-room in the cellar, and every room is provided with special means of ventilation independent of the windows. Water is supplied by an electric pump, which keeps a tank in the attic of the residence (the loftiest point on the grounds) full by an

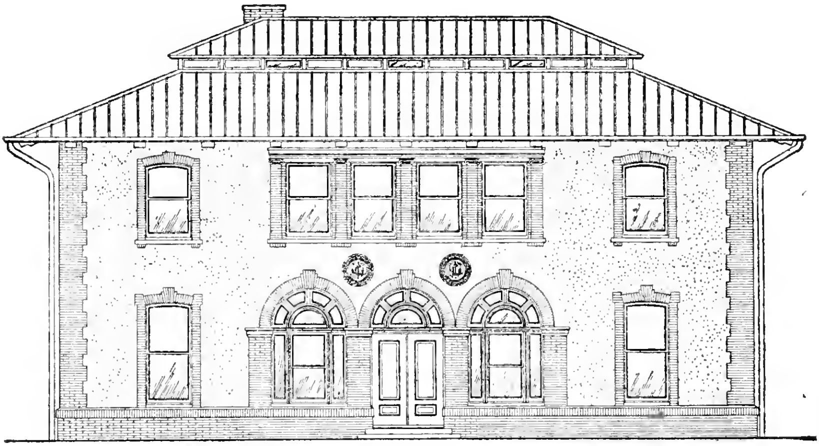


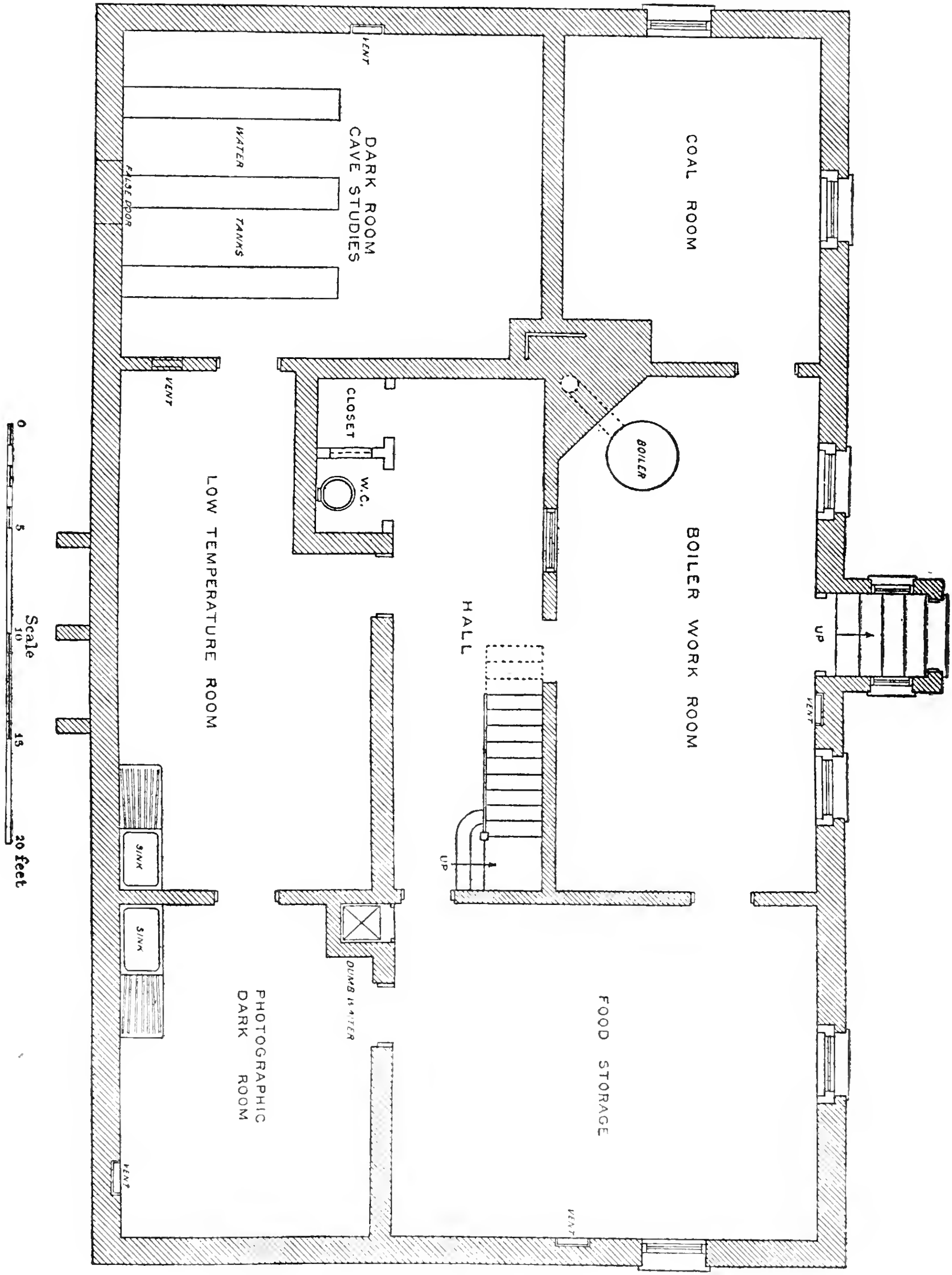
FIG. 2.—Cold Spring Harbor Station, west elevation.

automatic float-switch. The building is heated by steam, the temperature being automatically regulated.

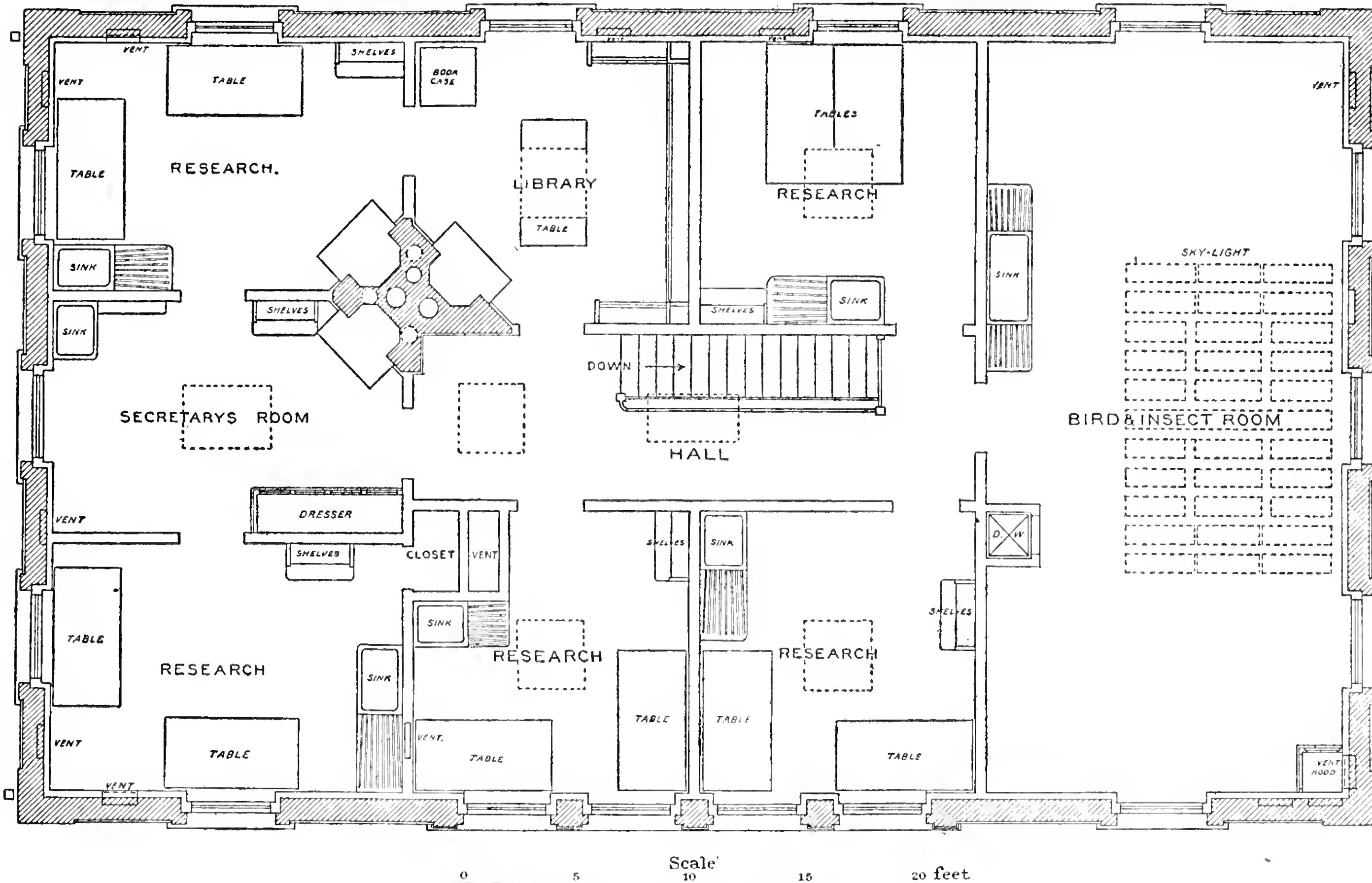
Three undertakings contemplated from the beginning will have to be deferred until 1905. These are: first, a plant-propagating house about 18 feet by 50 feet; second, a wire covering to the experimental garden to keep out seed-eating birds; and, third, a series of outdoor fish-ponds, involving 1,000 feet of piping from springs.

In addition to aids in correspondence and registering, such as a typewriter and letter and card files, the station possesses two compound microscopes and two dissecting microscopes, one Minot microtome, paraffin bath, the necessary glassware for cytological work, and a full laboratory equipment. We have also two adding machines for statistical work, a few meteorological instruments, an incubator, a

PLATE 2.




COLD SPRING HARBOR STATION, CELLAR PLAN.




COLD SPRING HARBOR STATION, SECOND FLOOR PLAN.

food grinder, an outfit of carpenter's tools, and agricultural implements. The station owns one horse, a farm wagon, a runabout, two sets of harness, and a 27-foot naphtha launch. We have recently purchased 17,000 brick, left over by the contractor on the building, and $100 worth of roofing material; we have on hand over $1,000 worth of lumber and $50 worth of paint.

*The Library.*—On account of the proximity of the station to New York city, it is not necessary that an attempt should be made to build up a general biological library. We have, however, collected a working library of compendia of the different sciences, books relating to experimental evolution, and complete files of a few important periodicals, and are taking some periodicals currently. Including those brought to the station by the writer, the following complete files are available: Allgemeine Zeitschrift für Entomologoie, American Journal of Physiology, Bericht der Deutschen Botanischen Gesellschaft, Biological Bulletin, Botanisches Centralblatt, Bulletin of the American Museum of Natural History (exclusive of vol. 1), Journal of Morphology, L'Année Biologique, Zoological Record, Zoologische Garten, Zoologischer Anzeiger; also complete series of zoölogical cards of the Concilium Bibliographicum to date.

The following periodicals are taken currently: American Naturalist, Biologisches Centralblatt, Breeder's Gazette, Bulletin de la Société Zoologique de France, Deutsche Landwirtschaftliche Presse, Experiment Station Record, Farm Journal, Gardener's Chronicle, Journal of Experimental Zoology, Journal of the Royal Horticultural Society, Nature, Nautilus, Popular Science Monthly, Psyche, Revue Générale de Botanique, Science, Zeitschrift für das landwirthschaftliche Versuchswesen in Österreich.

In addition to the books purchased by the station, the director has brought to the station 2,500 bound books and pamphlets, largely relating to general and experimental biology.

## ORGANIZATION.

The station comprises four classes of workers: (1) The resident staff and helpers, (2) honorary associates, (3) associates, (4) correspondents.

(1) The resident staff includes those engaged in the scientific work of the station and resident there. Besides the writer, whose work is largely on domestic animals, mollusca, and crustacea, the staff includes Dr. George H. Shull, whose work is chiefly botanical and who also has charge of the herbarium; Mr. Frank E. Lutz, whose work is chiefly on insects and who has charge of the record

of the periodical animal and meteorological phenomena; Miss Anne M. Lutz, whose work is partly on the cytological phenomena of heredity and partly keeping the administrative records of the correspondence of the station; and Miss Mabel E. Smallwood, who gives some time to the care of the library.

The helpers at the station include the following: Lewis Anderson, mechanician; John N. Johnson, animal caretaker and janitor; Thomas E. Kelly, gardener and general helper. These men are serving the station loyally and doing much to advance the work.

(2) *Honorary Associate.*—In recognition of the preëminence of his researches in experimental evolution and in commemoration of his participation in the opening of the station, there has been chosen as honorary associate in perpetuity Dr. Hugo de Vries, professor of botany and director of the botanic gardens, University of Amsterdam.

(3) *Associates.*—These are biologists who are either receiving some assistance through a grant directly from the Carnegie Institution in the Department of Experimental Biology for work similar to that done at the station, or receiving aid in their investigations directly from the station. They may be regarded as the non-resident staff of the station. Associates of the station confer with the director from time to time about their investigations in experimental evolution, to the end that there may be mutual understanding of work and avoidance of unnecessary duplication of work. Results of associates are submitted to the director of the station before publication in a medium mutually agreed upon. Associates are appointed for the calendar year. The following are associates for 1904:

Dr. Nathaniel L. Britton, director, New York Botanic Garden. Coöperation in experiments in mutation breeding.

Dr. William Ernest Castle, assistant professor of zoölogy, Harvard University. Studies in breeding mammals.

Dr. Henry Edward Crampton, professor of zoölogy, Columbia University. Selection in Lepidoptera.

Dr. Edward Laurens Mark, Hersey professor of anatomy, Harvard University. Cytological studies in Mendelian hybrids.

Dr. Daniel Trembly MacDougal, assistant director, New York Botanical Garden. Mutation in Onagra (Oenothera); coöperation in studies on plant mutation.

Dr. William J. Moenkhaus, assistant professor of physiology, Indiana University. Fish hybridization.

William Lawrence Tower, instructor in zoölogy, the University of Chicago. Evolution of the Colorado potato beetle.

Dr. Edmund Beecher Wilson, professor of zoölogy, Columbia University. Cytology of hybrids.

(4) *Correspondents.*—These are biologists in the United States and abroad who are engaged in experimental breeding and who have indicated their willingness to enter into the relation for the exchange of publications and correspondence upon matters of mutual interest.

The following have entered into this relation up to October 1, 1904:

Mr. William Bateson, University of Cambridge, England.
Dr. Alexander Graham Bell, Washington, D. C.
Dr. C. E. Correns, professor of botany, University, Leipzig.
Dr. Lucien Cuénot, professor of zoölogy, University, Nancy, France.
Dr. E. Fischer, Zurich, Switzerland.
Mr. C. C. Hurst, Leicester, England.
Dr. Arnold Lang, professor of zoölogy, University, Zurich, Switzerland.
Dr. M. Standfuss, professor of entomology, Zurich, Switzerland.
Dr. Erich Tschermak, Hochschule für Bodenkultur, Vienna.
Dr. Charles Otis Whitman, professor of zoölogy, University of Chicago.

## SCIENTIFIC WORK.

From the very nature of the investigations undertaken and the slowness with which most animals and plants breed, few scientific results can be expected from three or four months of work. Results are just beginning to come in, and will be published in the scientific series recording the work of the station.

It is the policy of the station not to undertake particular lines of experimental work that are being done well elsewhere, and consequently certain fields cultivated at universities are not undertaken, although the material might be especially favorable for results.

Dr. Davenport, in addition to a heavy burden of administration and his duties as director of the Biological Laboratory of the Brooklyn Institute, has devoted himself largely to breeding domesticated animals to test the range of validity of the theory of unit characteristics. In these experiments the station is receiving the coöperation of neighbors. Cows, sheep, goats, cats, poultry, and canary birds are being bred and especially cross-bred. Experiments are also being made upon wild species of crustacea and mollusca. Records are kept of the breeding periods of representatives of these groups. Particular attention is being paid to Japanese long-tailed fowl to test the cause for their peculiarity. Already certain results of hybridization have been obtained, of which a report will be made later.

Dr. Shull reports as follows:

The ground to be devoted to garden experiments had not been tilled for about 15 years, and the first problem which presented itself in preparation for botanical investigations was the conversion of this heavily sodded meadow-land into a successful garden plot. About three acres of ground were broken, and continuous cultivation has reduced the soil to a very satisfactory physical condition. With the exception of a few small areas, the soil, a light, sandy loam, has proved fairly fertile; thorough manuring will be needed, however, to bring it to the high degree of fertility desirable in a garden.

Owing to the uncertainty of results to be obtained under the conditions presented this year, none of the special cultures to which the garden is to be largely

devoted was undertaken here, but, through the generosity of the New York Botanical Garden, a number of species of plants were grown there from seeds collected by Dr. Hugo de Vries in Holland and sent by him for the use of the station. Several of these species appear to offer promising material and will be cultivated at the Station for Experimental Evolution next year. Other species will be discarded for various reasons, chiefly because of doubtful antecedents, as in the case of Iberis, Tagetes, and other common garden species, partly because of technical difficulties in the way of satisfactory characters for the observation or measure of variability.

The plants which have been grown in the garden to offer a sufficient immediate incentive for thorough cultivation have been ordinary garden crops. A portion of the products has been sold, resulting in a small revenue to the station, and the remainder has been used as provender for the various forms of animal life which are being reared. Some variations have been observed in these species, and seeds have been saved to test the heritability of these variations. Several species have been used also as a basis for experiments in hybridization.

It is the settled policy of the station, however, to devote its attention as far as possible to native plants, in order that results may not be vitiated by the effects of unknown garden treatment in the past history of the plants. One of the most important activities this season, therefore, has been the collection of the seeds of native plants for cultivation. In this work important assistance has been received from the New York Botanical Garden. Seeds of about one hundred species are now in hand. The aim has been to secure seeds representing as wide a range of natural orders as practicable, and an effort has been made to get, among others, a number of species whose normal habitats are diverse from those which will be presented at the station, with the hope of finding some which will tolerate the new conditions through the production of adaptive structural modifications. Accordingly, seeds have been collected on the oak and pine barrens of central Long Island and on the sand dunes of Fire Island beach and Bayville, Long Island; a few days were spent in the White Mountains, New Hampshire, collecting seeds of alpine plants. Through the kindness of Mr. Arthur Stanley Pease, Andover, Massachusetts, seeds of about twenty species were obtained from Gaspé & Co., Quebec, most of these species having a northern range.

Special attention has been given to securing seeds of species showing a notable degree of variability, and in these species the seeds of individual plants have been taken separately and the plants have been preserved as herbarium specimens, in order to allow comparison between the offspring and the parent plant. As many as fifteen types of one species have been thus isolated.

The herbarium of the station is planned to consist of four distinct sections. First, there will be a section devoted to the local flora of Cold Spring Harbor, including an area having a radius of ten or fifteen miles; second, the pedigreed plants used in tracing the origin and heritability of variations will be in the course of years the main section of the herbarium; a third section will contain seedlings and juvenile forms; and in the fourth section will be preserved all those aberrant forms which would be classed as abnormalities or monstrosities. Several hundreds of specimens have been collected this season, belonging most largely to the section devoted to the local flora, but supplying smaller numbers to each of the other sections.

In connection with the herbarium of pedigreed plants a card catalogue has been established, which gives the origin and history of each lot of seeds that has been cultivated or is to be cultivated in the garden. A system of numbering has been adopted that will not only identify each plant or each lot of seeds, but will also indicate the parentage.

A second card catalogue gives phenological data regarding the local flora, and when fully developed will serve as an index to the condition in which any species may be found at any given date.

Notes have been made on the variations of certain species in the local flora, and in several instances quantitative studies have been completed. Preparation has been made for the continuation of this work during the winter by collecting and preserving material either in alcohol or as pressed specimens.

The station has also collaborated with Dr. D. T. MacDougal and other members of the staff of the New York Botanical Garden in a study of *Onagra lamarckiana* and its mutants, and the results of this study will appear shortly as a publication from this station. Arrangements have been made to cultivate several of these species of Onagra at the Station for Experimental Evolution during the next few years in order to determine the exact relation of the mutants to their parent form and their agreement or disagreement with known laws of variation and heredity.

## Mr. Lutz reports as follows:

The summer was chiefly spent in breeding insects for the purpose of discovering suitable material for future work in the investigation of variation and inheritance. Incidentally a general collection was made of insects abundant in this locality, especially of such as bid fair to be advantageous for use in evolutionary studies. Material was also gathered for determining, if possible, the existence and strength of assortative mating among the Arthropods, and part of this was worked up preparatory to publication.

Experiments have been started with a view toward determining the cause of macropterism in short-winged species and the opposite condition in long-winged ones. Dimorphic species seem especially suited to the investigation of Mendelism, and it is hoped that this particular dimorphism may throw some light upon the much-discussed question of inheritance.

Hybridization experiments in several genera of insects have been attempted, in conjunction with Miss Anne M. Lutz, in order to determine the behavior of the paternal and maternal chromosomes respectively.

## Miss Lutz reports as follows:

As a preliminary step to the study of the germ plasms of hybrid plants and animals, it seemed advisable to spend a considerable portion of the present year in making a general survey of the field about us, with a view of discovering such forms native to this locality as might present desirable cytological qualities for future hybridization experiments. As material is gradually acquired, full data concerning it will be carefully recorded and the slides filed in cabinets under convenient heads for future reference. Some little has been accomplished in this line, other forms are the subject of present investigation, and considerable material has been acquired and preserved for winter study.

For several very obvious reasons, attention has been directed particularly to the study of insects. Material is abundant and in many instances readily obtainable; the appearance of several successive generations in the course of a summer is a further desirable feature; and, lastly, considerable literature on the spermatogenesis of insects is available for suggestive and comparative study.

Although I have relied in the main upon the efficacy of the osmic mixtures, Flemming's and Hermann's fluids for the best preservation of animal tissues, and of weak chromo-acetic for plants, no particular fixing agent can be relied upon for universal satisfaction. However, it seemed undesirable to consume much time during the collecting season in sectioning material and testing the relative merits of various reagents; consequently I have selected some three or four generally reliable fixatives, and wherever possible preserved such quantities of germ plasm in each of these that sufficient material for study may be obtained from any one that may later be found superior to the others. In order to insure the best results, I have hastened most of my objects through the grades of alcohol and into paraffin as rapidly as possible after fixation.

It is naturally to be anticipated that much of the work of the cytologist will apparently come to naught, as it may be presupposed that the chromosomes of closely related forms in the vast majority of cases will be found similar in size, shape, and number; but work will be continued independently and in connection with the experiments being carried on by other members of the staff, and if from among many failures an occasional result may be obtained which will throw new light upon the question of inheritance, the reward will be ample.

## PUBLICATIONS.

The results of the resident staff and associates of the station are to be published, it is expected, in the form of a series of studies under the general title "Scientific Results of the Station for Experimental Evolution." Already two papers, the first exclusively by an associate, Dr. W. E. Castle, and the second by another associate, Dr. MacDougal, in conjunction with Dr. George H. Shull and others, are ready for the printer.

## ACKNOWLEDGMENTS.

The station has a number of gifts to acknowledge in addition to many offers of assistance, some of which have already been taken advantage of. We have already referred to the supreme gift of the valuable land from the Wawepex Society.

From Dr. Alexander Graham Bell, three of his multinipple sheep.
From Dr. O. L. Jones, building sand and gravel.
From David Jones and Charles Jones, scientific books from the library of the late Edmund Jones.
From Mr. Timothy Treadwell, East Williston, one Hampshire Down ram at one-fourth value.
From P. Blackiston's Son & Co., publishers, two text-books on embryology.
From American Museum of Natural History, set of bulletins.

## ADDRESSES AT OPENING OF THE STATION FOR EXPERIMENTAL EVOLUTION, JUNE 11, 1904.

### INTRODUCTORY ADDRESS BY C. B. DAVENPORT.

LADIES AND GENTLEMEN: On behalf of the resident staff of the station I bid you welcome to our opening exercises. We do not celebrate here the completion of a building, we are dedicating no pile of bricks and lumber—rather, this day marks the coming together for the first time of the resident staff for their joint work, and we dedicate this bit of real earth, its sprouting plants and its breeding animals, here and now to the study of the laws of the evolution of organic beings.

Representatives of the Board of Trustees of the Carnegie Institution, we feel the full weight of the responsibility we accept in receiving the grant that you have made to this station. You have given us a fair start. It is for us to justify your confidence in us and the worthiness of the work to command continued and increasing support. However, as many of our experiments will demand years for their completion, quick returns must not be looked for. Without making big promises of things that we are going to do, we may state our confidence that important scientific results can be gained in the work that we have begun, and assure you that whatever devotion and scientific training can achieve we shall, up to the limit of our resources, do. We work, however, not alone, but with the assistance of our neighbors and scientific colleagues.

Gentlemen of the Wawepex Society, this celebration is yours. But for your generous proffer of the land intrusted to you by the late John D. Jones for the use of science, this station would never have been established here. Your appreciation of research has made possible the realization at Cold Spring Harbor of that dream of Bacon, who saw in the new Atlantis gardens devoted to the experimental modification and improvement of animals and plants. Your faith in our projected work increases the burden of our responsibility.

Gentlemen of the board of managers of the Biological Laboratory of the Brooklyn Institute of Arts and Sciences, this new station comes as a neighbor of your laboratory, glad to give and receive scientific companionship. We shall get stimulus from the enthusiastic students of nature who work at the laboratory each summer, and trust to recruit from them some who, as investigators, shall coöperate in the work of the station.

Neighbors, we have been already for some time acquainted, and if I have long desired to have the station stand in this community it was because I knew that you would appreciate our work and be glad to assist it. We have already received the largest confirmation of our belief. Generous proffers of use of land, of building materials, of coöperation on a larger or smaller scale, have come to us on every hand. The gift that calls forth additional gifts has unlimited possibilities, and already the Trustees of the Carnegie Institution have cause for self-congratulation on having selected as the site for this station a community of such intelligence, resources, and generosity.

Scientific colleagues, this station belongs to the men of science who can use it for the purpose to which it is dedicated. The staff are servants of biological science and seek its advancement—not their own. Rejoice with us for the new opportunity that has come to our science. We look to you for collaboration, for coöperation, and for criticism and advice. With such assistance, this station must succeed in achieving the ends for which it is founded.

## Address of Presentation by W. R. T. Jones, Governor of the Wawepex Society.

Representatives of the Carnegie Institution of Washington, ladies, and gentlemen:

Cold Spring has experienced several distinct changes since Prime, in 1845, wrote his history of Long Island. He devoted to it just four lines, describing it as "a considerable village in the northwest corner of the town (Huntington), lying on a harbor known by the same name." The village had long possessed two factories and a flour-mill, which were of great benefit to the neighboring farmers in taking their wool and grinding their grain; also two or three stores, all doing a small paying business. With the introduction of the whale-fishery business the village awoke to a real boom. Buildings were erected to accommodate this business, houses built for the employees, and in my early days the village, especially on the west side, showed its activity by noises from the continued hammering of iron, the resounding echo from the coopering shops, the clanging of boat-builders, and the buzzing of saws. When this business became no longer profitable, the place soon appeared like a deserted village—houses became vacant, buildings unused, and everywhere neglect and decay.

The whale-ships ordinarily came to anchor in the outer harbor. My father, John H. Jones, built a dock on the east side of the inner harbor to facilitate their outfitting, and I have seen a vessel fitting out at that dock for a three years' voyage to the Arctic; but the great rise and fall of the tide prevented the experiment being a success, and the original anchorage was resumed. The great rise of the tide—some 7 feet—was in one respect an aid outside, for, lying at anchor several months, the anchors sank so deep in the mud that the windlasses of the vessels could not start them, and when the chains were hauled taut for the vessel to pull by the rise of the tide, it often took several tides before the windlasses could weigh anchor, necessitating three days in breaking anchorage.

There were two post-offices by the name of Cold Spring in this State, and the delivery of letters became so confused between the one on the North River and the one on Long Island that the name of the Long Island village was changed to Cold Spring Harbor. It was then made a port of entry, an honor which I believe it still retains, but the income is very limited. Many of the deserted buildings were torn down—one because it interfered with the view of the outer harbor from this house; two or three have been modified so as to be of present use. The inner harbor, with its clear water, was in those days a constant source of amusement. A pretty sandy shore at the lower end of these grounds, with a clean sand-bar extending out, was a delightful place for youngsters, especially from the district school near by, to bathe at medium tide, and I never failed in taking advantage of this sport. A legend was long current that General Washington, on his way from Oyster Bay through the island, halted at this school-house when being erected and gave personal aid in raising the first rafter. At low tide the water largely covered the bottom, and at the deep hole a number of acres were always filled with 5 to 6 feet of water, even at the lowest tide, which permitted a pleasant pastime for young people to fish and secure results worth serving at the table, the incoming tide always bringing in a fresh supply of fish. Occasionally, but at long intervals, one or two porpoises might be seen sporting in the inside water, but as soon as the tide turned to ebb they made for the outer harbor and no effort to stop them ever succeeded, as they dived under or leaped over the string of boats stretched across the narrow entrance to stop their escape.

The next change, particularly on the west side, assumed a scientific aspect.

My brother, John D. Jones, inherited the family homestead and adjoining grounds. He was born in the family mansion, which was destroyed by fire, and he erected this building on the site of the old house. The Brooklyn Institute desiring a place to establish a school of biology, he put up for that institute a building suitable for its purpose, and the school, under charge of able professors, has been a success, doing original work which has been a credit to Long Island, and acknowledged as such by similar foreign institutions. He also leased to the State of New York grounds for a fish hatchery, which is now turning out each year several hundred thousand trout and salmon to stock the inland waters of the State.

Seeing the need of an organization to perpetuate the management and care of the grounds and property devoted by him to scientific research, he incorporated the Wawepex Society under the laws of the State of New York governing scientific societies, and the above society has been in charge for several years. The name is taken from an old Indian name of the harbor. Mr. Jones, one of the incorporators of the society, at its meeting January 25, 1892, to organize, was chosen as governor, and was continued in that office until his death, September 22, 1895.

This year the Carnegie Institution, attracted by the advantages of the locality, has asked for a fifty-years' lease of part of the grounds, taking in this house, for carrying out experiments in evolution, promising to put up a special building for that purpose, and the lease has been granted. It gives great pleasure to the Wawepex Society to pass over to the representatives of the Carnegie Institution the papers putting that institution in possession of as much of the property as it desires for erecting buildings to carry out its experiments. I trust in going back and investigating, as far as possible, the origin and order in creation it will find nothing to interfere with the doctrine of the church just around the corner, erected largely by aid of family relatives, in its efforts for improving morals and explaining to the best of its ability life hereafter.

With these three institutions hailing from our village, it will assuredly soon become well known and appreciated both at home and abroad.

REMARKS BY DR. JOHN S. BILLINGS, U. S. A., CHAIRMAN OF THE BOARD OF TRUSTEES OF THE CARNEGIE INSTITUTION OF WASHINGTON.

It gives me great pleasure to accept, in behalf of the Trustees of the Carnegie Institution of Washington, the offer of the Wawepex Society to grant to us the use of these grounds for the establishment of a Station for Experimental Evolution, and I beg to offer our sincere thanks for, and the assurance of our high appreciation of, this important and valuable grant.

In considering the numerous applications for grants of money for research which are made to the Carnegie Institution, we have been in the habit of asking several questions: First, Is the proposed research one that will probably give good results? Second: Is it a research which any individual or institution is carrying on, or is likely to undertake? Third, Who is the man who proposes to undertake it, and what are his qualifications? Fourth, Is it an individual piece of work, or does it involve coöperation?

Among the first recommendations made to the Carnegie Institution for research in biology were several advising the establishment of an institution for the study of heredity, development, and evolution by experimental methods. It was evident that such study, if properly made, would give interesting results which might be of great practical importance, but that if the work were undertaken it must be with the distinct understanding that it should be continued for a long period.

We took a year to make further inquiries, from which it appeared that no person or institution was likely to undertake such a work as this, although there were a number of persons in this country and in Europe who were engaged in research upon various points connected with the general subjects of evolution and heredity.

We also found that there was a man who was willing and anxious to take charge of the work—a competent man who had demonstrated his ability, an exceptional man willing to give his life to the researches proposed.

We found that these researches could not be carried out as they should be carried out by any individual; that they require coöperation and coördination of results; that it is desirable that many students should be engaged on different sections of the problem, and that these students, each working in his or her own way, should be aided as far as possible by this department.

In view of these facts we decided that a portion of the funds intrusted to us by Mr. Carnegie to encourage investigation, research, and discovery should be devoted to a Department of Experimental Biology, a main feature of which should be the establishment of a station for the study of experimental evolution, to be located here at Cold Spring Harbor, and it is this station that we are inaugurating to-day.

We know that experimental investigation, especially in this field, is a slow process, and uncertain in its results, and that we must be patient. This is a seed that we are planting; for the buds and blossoms and fruits we must wait, believing that they will come in due season, although they will probably not be what we now expect.

The scope of the work of this department of experimental biology is wide and far-reaching. Already the results of biological research have had a strong influence on philosophy and theology, and we can hardly even imagine what the outcome may be in sociology and political science.

The problems of evolution and development through heredity involve the structure and functions of that part of the living organism which seems to be necessary for what we call mental action, from the lowest, dimmest forms of consciousness, through memory and will to the highest flights of art, philosophy, poetry, and religion.

Let us hope that the work of this station will be so well done that by the time it celebrates its fiftieth anniversary it will have demonstrated the wisdom of its establishment.

Prof. Franklin W. Hooper, director of the Brooklyn Institute of Arts and Sciences and secretary of the board of managers of its biological laboratory, located on the ground adjacent to the new station, next spoke. He regretted the absence of Mr. Eugene G. Blackford, president of the board of managers of the laboratory, due to illness, and welcomed the new station as a neighbor of the biological laboratory.

Mr. Davenport, in introducing Prof. H. de Vries, said:

I have before me two or three books: One, by Professor Weismann, dealing with the "Germ Plasm," presents the great guiding theory of the development of the individual. But the foundations of this theory were laid some years before Weismann, in a little work entitled "Intracellular Pangenesis," from which work, consequently, the modern science of cytological embryology dates.

Every one knows of the great revolution wrought in physics and chemistry by the new science of physical chemistry. One of the

most far-reaching generalizations of this science is that of solutions. The first to investigate this subject was not a chemist, but a botanist, the author of "Intracellular Pangenesis," who is therefore one of the founders of physical chemistry.

During the last three years this great work that I hold in my hand has appeared, entitled "Die Mutationstheorie," the most important work on evolution since Darwin's "Origin of Species," a work destined to be the foundation stone of the rising science of experimental evolution. It also is by the author of "Intracellular Pangenesis."

To be the author of any one of these works establishing a science is to be famous. It is an exceptional opportunity that we have to meet the preëminent author of all three, Dr. Hugo de Vries, professor of botany at the University of Amsterdam and director of its botanical garden.

## THE AIM OF EXPERIMENTAL EVOLUTION, BY DR. HUGO DE VRIES.

LADIES AND GENTLEMEN: A bright prospect opens before us. Hopeful preparations have been made to start on a new course. Strenuous endeavors are proposed to wrest from nature secrets which not long ago seemed almost impregnable. The matter of the evolution of organic life on this earth, hitherto a subject of great admiration, admitting only of appreciative and comparative studies, is to be investigated to its very core. We are no longer content to look at it in a broad way, to enjoy the mighty display of harmony between all living beings and to sit down and wonder. We want to have a share in the work of evolution, since we partake of the fruit. We want even to shape the work, in order to get still better fruits.

Evolution must become an experimental science. First it must be controlled and studied, afterwards conducted along selected lines, and finally shaped to the use of man. To do this work you have called the man that was the first in this country to propose the hazardous combination, "Experimental Morphology," thus giving an impulse to a new direction of thought. No reward can be more satisfactory to a man of science than the opportunity to continue his researches on a large scale and with all the means required for success.

This opportunity is solemnly offered to-day. Mine is the task of congratulating the director and the staff of the new laboratory on this occasion and wishing them the success they so well deserve.

With all my heart I accept this responsibility. American science is rapidly gaining a prominent place in the esteem of Europe. More and more our eyes are turned westward. Important discoveries on fecundation, on sexuality, on the microscopic representatives of the heredity qualities, on systematic relationships, and on numerous other subjects contributory to the great science of evolution have of late been made in America. The honor you are this day bestowing upon me I appreciate very largely, because it implies the desire to fraternize. No words are needed to assert that this desire is perfectly reciprocal.

In trying to sketch for you my conception of the aim and work of this new laboratory, allow me to use a metaphor. Science is a source of light amid almost universal darkness. Brightly it shines on mankind, delivering us from ignorance and impotence, from doubt and fear. The light has to be kept bright; but, moreover, the field of its influence must steadily be enlarged. Hundreds and thousands of industrious men are engaged in this work. Large numbers of scientific institutions provide the means and direct the efforts. On all sides the illuminated area is being extended, increasing the blessings of knowledge.

Besides this assured and systematic progress another method is from time to time adopted. Centers of illumination are thrown out far away into the surrounding darkness, constituting new starting points from which to win dominion. Often they become extinguished, leaving no trace of their existence, but sometimes persist and glow. In these cases the small point of light vigorously increases, and all the territory intervening between the new and the greater field of light becomes in time illuminated. Science is a mighty means of broadening our conceptions and our ideas, as well as our power to utilize the laws and materials of nature. Such new centers of illumination are the great landmarks of its progress. They immortalize the names of their founders. Bacon and Newton, Lyell and Darwin stand preëminent among all. Edison and Marconi, Röntgen and Curie are adding their genius to the universal effort.

With this lofty conception of a twofold method of scientific progress the Carnegie Institution fully complies. At Washington it is working toward a general increase of knowledge. Besides this, it has thrown out a first center of illumination far away into the arid desert to emit the rays of science and inquiry over phenomena not yet understood and over fields apparently uninhabitable and useless to man. MacDougal, Coville, and Cannon are guiding the work,

and under such promising auspices the light can not fail to increase and soon to shine brightly all about.

A second lighthouse is being established to-day. It is to be a beacon in quite another territory, illuminating the far more arid problems of the origin of species. It is surrounded by a denser darkness, for there is less previous knowledge in this field. It requires the care of a keeper thoroughly prepared for the work and of large experience. With him it will open up wide fields of unexpected facts, bringing to light new methods of improvement of our domestic animals and plants. The care of the lighthouse is given into the hands of Mr. Davenport and his staff, and many details of its internal affairs are looked after by the kind care of Mrs. Davenport. Thus provided, it can not fail to fulfil its mission, and to yield the results expected from it, and even more.

What these results will mean is as yet impossible to predict. Discoveries come unexpectedly; but as a rule they fall into the lap of those only who are prepared to make the most of them. Expectations, on the other hand, may be elaborated, and I consider it my duty to explain to you the nature of the expectations that the foundation of this laboratory is awakening in me. Of course only general outlines can be given, and the picture is to be painted with a broad brush in order to give an adequate image of what may some time be; but in the meantime I am fully convinced that the future will largely exceed even our highest hopes.

In conformity with the idea of the twofold methods of scientific progress, I imagine that this station, too, will work after these principles. The territory around the new center of light must be more and more completely illuminated. Besides that, beacons have to be carried forward into the darkness, and search-lights have to guide the progress along new paths.

What may be discovered by such search-lights can hardly be guessed at. It is quite a dream, a mixture of hopes and possibilities, of facts and hypotheses. What is real is the endeavor to get at the most intimate causes of evolution. I have indulged in this most delightful dream, and if you will allow me to give you a sketch of what I have seen, I may perhaps succeed in conveying to you an idea of what seems to me the farthest limits of inquiry for the present.

My dream started from the old question, What is that in the egg which enables it to develop all the qualities of the bird? Something must be there, and we may even assume that all the separate qualities displayed by the bird have their representatives in the egg.

Now, if it were only possible to get at these representative particles within the egg, what changes might not be effected in the development of the bird! To take a very simple example, the peacock has a white variety, lacking the bright colors of the feathers. If in the egg of an ordinary peacock we could seize upon the representative particles of the color and impede their development, perhaps we would succeed in reproducing the white variety at once and quite artificially.

Obviously this is the heart of the matter, for if once the principle should be discovered to dislocate such a representative, we might apply it to numerous other instances. A white peacock would be no novelty and no gain, but we would be able to make white varieties of other birds and other animals, and perhaps even of the bright-colored flowers, which until now have resisted all endeavors of the breeders in this line of work.

The white-color varieties are, of course, only intended as an example. Other and more valuable qualities might likewise be expected to become changeable. There would be no limit of success if that principle were found, and why should it not be possible to discover it? Methods of attacking this question are not at all failing. We might try to kill some of the representative particles in the egg, or to stun them, or to injure them in ever so slight a measure, so as only to retard their development. More than one starting point for such an attempt is at hand. Engelmann has taught us a method of lighting and heating small parts of a living cell. He uses the focal point of a glass lens, which he directs upon the cell while lying under the microscope. If now a very small part is overheated and thereby killed, the remainder of the cell is seen to be still living and apparently uninjured. By refining this method some of the most sensible representative particles might perhaps be killed without too much injury to the others.

Johannsen has of late discovered that plants may be stimulated by a treatment with narcotizing substances, such as ether and chloroform. Dormant buds may be awakened and display their leaves and blossoms even in midwinter. The studies of Overton have thrown considerable light upon the agency of such narcotizing substances upon the living protoplasm. Wilson has proved that visible changes may be effected in the eggs by means of ether. Though these observations seem to justify a hope of success, very much remains to be done. If we assume that some representative particles

are more sensible to ether than others, perhaps some could be made inactive, and the qualities they represent would fail in the development of the organism.

Loeb, of the University of California, has shown that the stimulus which fecundation gives to growth, besides and above the mixture of the hereditary qualities of the parents, may be replaced by purely chemical agents. He pointed out that the unfertilized egg remains inactive through the action of some unknown cause, which may be removed by the use of distinct salts.

Delage has markedly improved this method by making use of carbonic acid instead of salts, and it seems highly probable that by this or other gaseous agents the representative particles of the hereditary qualities might be attacked separately.

Davenport has studied the effect of poisonous chemical substances upon the growth of organisms, and shown that by gradually subjecting them to various poisons they become immune to them. Applying this principle to the representative particles in the egg, we might expect to find some immune while others were not, and thus to remove distinct peculiarities from the ensuing process of evolution.

Other agencies might be tried. The finest and most effective methods offered by allied sciences have to be made use of. If one way fails, another may succeed. The rays discovered by Röntgen and the radio-activity of the new element, radium, have already proved themselves capable of provoking important changes in living organisms. These changes are partly of a retarding nature, and some processes are more sensible to them than others. If the same holds good for our dormant representatives in the egg, we may hope some day to apply the physiological activity of the rays of Röntgen and Curie to experimental morphology.

Be this as it may, it is only a dream. Perhaps I have recalled to your mind too many facts and discoveries in too short a time. My object was only to convey to you the idea that the future work of this laboratory must keep in close relation with all the great victories of the sister sciences. It has to keep up with the newest researches and to omit not even the slightest occasion of profiting by the work of others. All sciences converge toward one main point, and any noticeable advance in one direction must obviously favor the work on the other lines. Opportunities of rapid success are not rarely offered, but the success really comes only to him who is steadily on the lookout for a chance and who is thoroughly prepared to profit by it.

Leaving these chances, we may now turn to the daily work. It is that work which calmly and steadily increases our knowledge and which is the most assured way to success, even if the advance is less striking and seemingly slower than in the alluring experiments alluded to.

The process of the evolution of animals and plants has to be attacked by direct experiment. This evolution, however, has a long history, covering many millions of years. Its historical part, of course, is not accessible to experimental work. From its innermost nature it must be studied according to historical and comparative methods. In laboratory work we may simply pass it by.

After eliminating this great mass of detail concerning the pedigree of the animal and vegetable kingdom, two points remain, which present themselves for experimental study. These are the beginning and the end. Obviously the real end is not yet reached, evolution going even now steadily on. In the same way we may assume that the beginning is not yet finished. The laws that ruled the material world some twenty or thirty millions of years ago must have been the same that are still ruling it in our days. Circumstances may have changed, but it is not very probable that those which permitted life at the beginning and those which have made it possible during the long geological ages should have been widely different. Quite on the contrary, it seems only natural to assume that new life may nowadays originate as well as in former times. It is only a question of where we are to look for it.

On this very difficult point I like to be guided by the genial conceptions of Brooks. In his "Foundations of Zoology" he depicts the primeval seas and their living population. All life must have been limited at those early periods to the high sea; all organisms were floating amid the waves, going only to a depth of some few meters. Here the main lines of the animal and vegetable pedigree must have been produced, starting the great divisions of both kingdoms. The only exceptions are offered by the flowering plants and the vertebrate animals, which seem to have originated on the shores or perhaps on the land itself. As long as all life was in this floating condition, evolution proceeded rapidly and broadened out. Then came a period when, as Brooks says, the organic world made the discovery of the possibility of living on the bottom of the sea, feeding on the sinking remains of the floating world. This great change was the starting point for numerous adaptations and for the evolution of a richness of forms and structures, but without the previous progress in the production of many really new divisions.

It is a very attractive image, and I much regret not to be allowed to follow it any longer. For us it points to the probability that the very first organisms must have been inhabitants of the high sea, floating in the waves; or, as it is now called, they must have been members of the plankton. Thence the conclusion that it is within the plankton that new creations are to be sought for. If really they are still occurring in our days, it must be the high sea that conceals them. Obviously these first organisms must have had the lowest possible degree of organization. They were not cells, they can not have had any differentiation. They must have consisted of a uniform jelly, with only the capacity of increasing their mass. If such a jelly could be detected, what possibilities would not be opened to experiments on evolution! The chance may seem very small, but then, before Röntgen and Curie there was no chance at all of discovering X-rays and radio-activity. The plankton has to become one of the main points of interest for all who care for experimental evolution.

The other end of the evolutionary development is the evolution that is still now going on. Here we are on a more assured ground, though even here the methods and the starting points have yet to be discovered. These, however, may be attained by strenuous work, attacking palpable phenomena from obvious sides, and subjecting them to the general methods of ordinary experimental inquiries.

Two main lines have to be followed. One is the direct study of variability; the other relates to the dependency of this variability on the outer conditions of life. The first line uses the statistical methods, while the second relies chiefly on the experiment. Both have to be cultivated as well on botanical as on zoölogical ground. Four large divisions are here indicated for the daily work of the laboratory; but it is a manifest advantage that the leader of the work should be conversant with all of them. Mathematical and statistical studies have their eminent representatives in Europe, both among zoölogists and among botanists, and likewise experimental work has not been neglected by them; but none of them combines the severe requirements of mathematics and statistics with the looser methods of morphological inquiry, and with the strict rules of experiment, and this as well in the study of animals as in that of plants. Such wide erudition and large experience, however, are preëminently necessary in the man who has to take the direction of this new laboratory, and it is from the innermost core of my

heart that I congratulate you on the good fortune that made you find this combination in the appointed director.

Fluctuating variability, however, has been the chief line of study for Mr. Davenport, and he would be a bold man who would try to show the way where such a guide is at hand.

For my part I prefer confining myself to such questions as are more obviously touched by my own line of work. The experience of agriculturists and horticulturists has long since established the fact that new forms of animals and plants from time to time arise. How they originate is another question, which it is not the task of practice, but of science, to answer. The fact, however, is undeniable, and all observations point to sudden changes or so-called sports as the first beginning. Especially in the dominion of horticulture Korshinsky has shown, by an ample critical survey of the historical evidence, that sudden sports are the prevailing rule and probably even the exclusive manner of originating of new varieties.

Such considerations have led to the conviction that what occurs in horticulture must also occur in the experimental garden. If the conditions are the same, why should not the phenomena be the same, too? If mutations are rare in horticulture, the experimenter has only to arrange his work so as to be able to detect rare occurrences in his cultures, too. In doing this I have succeeded in observing mutations quite analogous to the horticultural instances, and collecting all the evidence concerning their ancestry and their descendants as well as concerning the mode of their appearance.

Moreover, I have had the good fortune of discovering a wild plant which is even yet in a condition of mutability. Yearly it is observed to produce new species. It is the large-flowered evening primrose, which bears the name of Lamarck, the founder of the theory of evolution. It clearly shows how new species arise from an old stock, not by continuous and slow changes, but all of a sudden. The stock itself is not altered by the process nor even noticeably diminished. The new species which it produces arise on all sides. Some of them are in a higher, others in a lesser degree fit for their life conditions; some persist during years, while others disappear nearly as soon as they arise.

This instance of experimental mutations is found largely to agree with the experience of breeders, especially in horticulture, and likewise with the conclusions that have been drawn from comparative studies. The assumption that those species and genera which now consist of large groups of closely allied forms have originated in

the same way seems quite undeniable; and as soon as the validity of this generalization is granted for these cases it will have to be considered of general, if not universal, bearing.

It is chiefly owing to the work of Mr. MacDougal that the evening primroses have come to be recognized in America as the true material for the study of evolution by sudden leaps. His cultures of the original stock and some of its mutants have proven the significance of the differences between the new and the old species, and have awakened an increasing interest in this line of research. To the demands made by such work the laboratory has to respond, and it is now my duty to point out the chief lines which should be followed in order to reach this aim.

Two main lines have to be distinguished: the study of the phenomenon itself and that of its causes. Mutations, of course, can not be assumed to be a special feature of the evening primroses. They must occur elsewhere, too, and these have to be sought for. The Oenothera was one of a lot of nearly a hundred species tested as to their constancy; it proved to be the only changeable form among them. By testing a hundred other species or other strains of the same forms it seems probable that one or two new instances of mutability may be detected. The best way is to try the wild species of the nearest environments or of other regions with a corresponding climate, since large numbers of seedlings have to be examined. One or two novelties among thousands of individuals of the common type are not easily found, especially when the differences are slight and new, and thereby apt to be overlooked. Much care is to be given, and the trials have to be repeated with the same species in succeeding years. With increasing experience the chances of discerning the small indications of novelties are rapidly augmented. No differentiating marks, however slight, should be considered as insignificant. All aberrant individuals should be planted separately and protected with all the care required to insure the fullest development. Many of them afterwards prove to be only fluctuating variants or to have deceived the experimenter. They are simply discarded. It is quite sufficient if some remain and prove to be mutants. As soon as in this manner a mutable strain will be discovered the greater part of the other species may be excluded, although the search for new mutable species should never be wholly neglected. Each year some new forms should be taken into culture, in order to have sufficient chances of gradually increasing the evidence concerning the occurrence of mutability in nature.

The chief object of this inquiry, however, must be the study of the mutable strain itself. Some of its seeds yield new species, while others are more conservative. Thence the question, Which seeds mutate, and by which causes are they elected to do so? The location of the mutating seeds within the fruit, the position of the preferred fruits on the spikes, the influence of the individual strength of the sundry branches, and many other points have to be investigated. Further, it is probable that the degree of mutability, or, in other words, the yield of mutating seeds, is more or less dependent on the outer life-conditions. Thence the necessity of studying the influence of culture in general, of light and heat, of soil and water, and last, but not least, of manure. Extreme combinations of these factors should be tried to see whether perhaps they may give extreme results.

Underlying all and directing all the efforts should be the hope of obtaining such a knowledge of the phenomenon as would enable us to take the whole guidance of it into our own hands.

Obviously, this aim lies within the possibilities of the first series of years. Exact methods of working, severe isolation of the single individuals, artificial fecundation with complete exclusion of the visits of insects, and above all the great principle of individual seed-saving and seed-sowing, have to be the guides. Following the lines which are indicated by these prescriptions, gradually a power will be developed which will first enable us to increase the number of mutating seeds and afterwards to widen the range of mutability. New and unexpected species will then arise, and methods will be discovered which might be applied to garden plants and vegetables, and perhaps even to agricultural crops, in order to induce them to yield still more useful novelties.

Increase of knowledge of all the peculiarities which accompany the phenomenon of mutability is the most immediate requirement. On the foundation of the study of one single instance this increase can not be sufficiently broad. Other cases may display other features, and the problem is to be attacked from different sides. A broad foundation knowledge of phenomena is the most assured way to success.

Ladies and gentlemen, it is a high honor for me that this laboratory has been founded, and that the members of the board and the director have invited me to be its godfather. During a long series of years I have fostered my conception of sudden mutability and cultivated my primroses for myself and for myself only. Nobody

knew about them. I loved them and cared for them and enjoyed the security of perfect secrecy. It was the full quietness of pure scientific research. Of course I had the hope of doing something that might prove useful to science, but I lived in the conviction that many years, and perhaps a whole lifetime, were needed to reach so great a result. I felt myself secure and at ease, since there was no fear that anybody could infringe upon my work. The chance of a discovery of my primroses and of their curious qualities by anybody else seemed too small, because of the concealed position of the original locality.

Some years ago I allowed myself to be induced to betray my secret and to deliver it to the scientific world. It has at once been taken up by your countrymen, and the foundation of this laboratory is the mightiest and most dreadful competition that I could have. I have to give up security and freedom, quietness and calmness, and all that secrecy which I so dearly loved. I have to submit to the prospect of being soon surpassed and largely excelled on the path which until now I considered as my own. I have to yield my much beloved child.

But I do it gladly and without regret. It is the interest of the child itself which commands me. It will be better in your hands, Mr. and Mrs. Davenport, and in yours, lady and gentleman officers of the staff. Pray have good care of it and educate it assiduously, that it may become one of the most brilliant parts of your work, a glory to this laboratory and to the institution that founded it, a pride to your country, and a bliss for humanity.

## MARINE BIOLOGICAL LABORATORY AT TORTUGAS, FLORIDA.

### FIRST REPORT OF PROGRESS.

BY ALFRED GOLDSBOROUGH MAYER.

The Executive Committee of the Carnegie Institution of Washington having authorized the establishment of a laboratory for the study of marine biology at Tortugas, Florida, I have the honor to report as follows upon the results attained.

The director was unable to assume active charge of the work until June 1, 1904.

The Department of Commerce and Labor and the U. S. Light-House Board generously granted to the Carnegie Institution a license for a suitable site for the laboratory upon Loggerhead Key, Tortugas, Florida, and in this connection the director wishes to express on behalf of the laboratory his appreciation of the liberal spirit displayed by Hon. George B. Cortelyou, Secretary of the Department of Commerce and Labor; Major W. E. Craighill, U. S. A., engineer of the seventh and eighth light-house districts; and Lieut. Col. W. D. Lockwood, engineer secretary of the U. S. Light-House Board.

After consultation with Dr. John S. Billings and Dr. Charles D. Walcott, members of the Executive Committee, as well as with Prof. Charles B. Davenport, Edmund B. Wilson, Charles H. Townsend, and others, it was determined to erect large but portable laboratory buildings, which should be designed especially to be cool, well lighted, and capable of affording to a limited number of investigators unrivaled facilities for the study of the marine life of the tropical Atlantic.

It was decided to erect a main laboratory, one small detached laboratory, a kitchen, a windmill for pumping salt water and air, a dock, a shipways, two small outhouses, and a cistern for rain-water.

The main laboratory, small laboratory, and two outhouses were constructed by the Drecker Company of New York, and are portable, so that they can readily be moved from their present site and re-erected elsewhere if desirable.

These buildings were erected in July, upon the western side of Loggerhead Key, more than 1,000 feet north of the light-house. The ground was cleared of trees during the last week in June and all necessary grading accomplished. About 50 tropical palms were planted upon the cleared ground, in order to shade the buildings, afford protection in the event of hurricanes, and beautify the site.

The laboratory buildings were carried by steamer from New York to Key West, and thence to Loggerhead Key upon a schooner of

PLATE 4.

THE MARINE BIOLOGICAL LABORATORY AT TORTUGAS, FLORIDA, JULY 28, 1904.

light draft, as it was necessary to land the buildings, although no dock was available. This was accomplished without the least accident, although a period of baffling calms caused a delay of more than two weeks in sailing from Key West to Tortugas. A good supply of laboratory glassware, chemicals, apparatus, and furniture

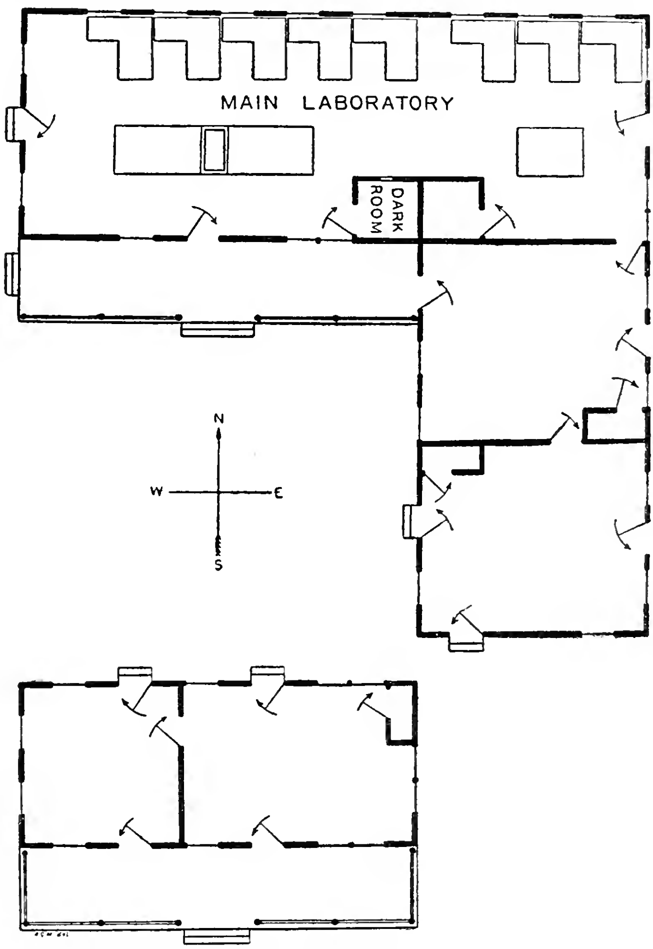



FIG. 3.—Plan of Laboratory Buildings at Tortugas, Florida.

was also landed, it being deemed desirable to take advantage of the calm period preceding the hurricane season in order to accomplish this purpose; 500 feet of iron rails, a powerful capstan, water-pipes, and lumber for dock and shelving were also safely landed, and the dock, which is 84 feet long, was completed in August.

The main laboratory is L-shaped and is 106 feet long. It is one story high, and the roof contains eleven ventilating traps, thus rendering the building remarkably cool even on calm, hot days. The laboratory-room proper is 53 feet long and 19½ feet wide, and contains a dark-room, a large closet, and ample accommodations for eight investigators, each of whom will have an L-shaped microscope table facing the north light.

In order to resist hurricanes, the laboratory buildings and the dock are very strongly braced, and the foundation posts are all T-shaped on their sunken ends, thus rendering it well-nigh impossible to overturn the structures.

The entire cost of the main laboratory, small laboratory, and two outhouses, including cost of clearing and grading ground, hire and maintenance of workmen, and payment of transportation and insurance from New York to Tortugas, was $4,806.13.

The director completed a survey of the site and reported upon the same to Major W. E. Craighill. In answer to the petition of the director, the U. S. Light-House Board granted permission to erect the shipways in the situation shown on the survey map, this being at the place where the last suitable tract of beach rock is found on the northwestern side of the island. A shipways will be necessary in order to draw out the laboratory vessel in case of hurricane.

The director made numerous surface tours while at the Tortugas, and the results of this work will be presented for publication in connection with an investigation of the entire Atlantic coast from Maine to Florida, opportunity for the study of which will be afforded by the laboratory vessel.

In order to study the marine life of the tropical Atlantic, using the Tortugas as a land station, it is essential that the laboratory should be provided with a stanch, sea-going vessel of light draft, capable of making headway against the strong currents of the coral reefs and the Gulf Stream. Such a vessel was designed by Stearns & McKay, of the Marblehead Yacht Yard, Marblehead, Massachusetts, and on April 28 they were commissioned to direct the Rice Bros. Company, of East Boothbay, Maine, to construct the vessel.

The design called for an auxiliary ketch 57 feet over all, 15 feet 11 inches beam, and 3 feet 6 inches draft, to be heavily and strongly built in order to withstand tropical hurricanes, and to be provided with a 20-horsepower Motor Engine Co. naphtha engine. The hull is of wood, copper-bottomed, with a heavy iron keel and two centerboards. There are accommodations for seven men, and the vessel is especially designed to dredge in depths of 500 fathoms or less.

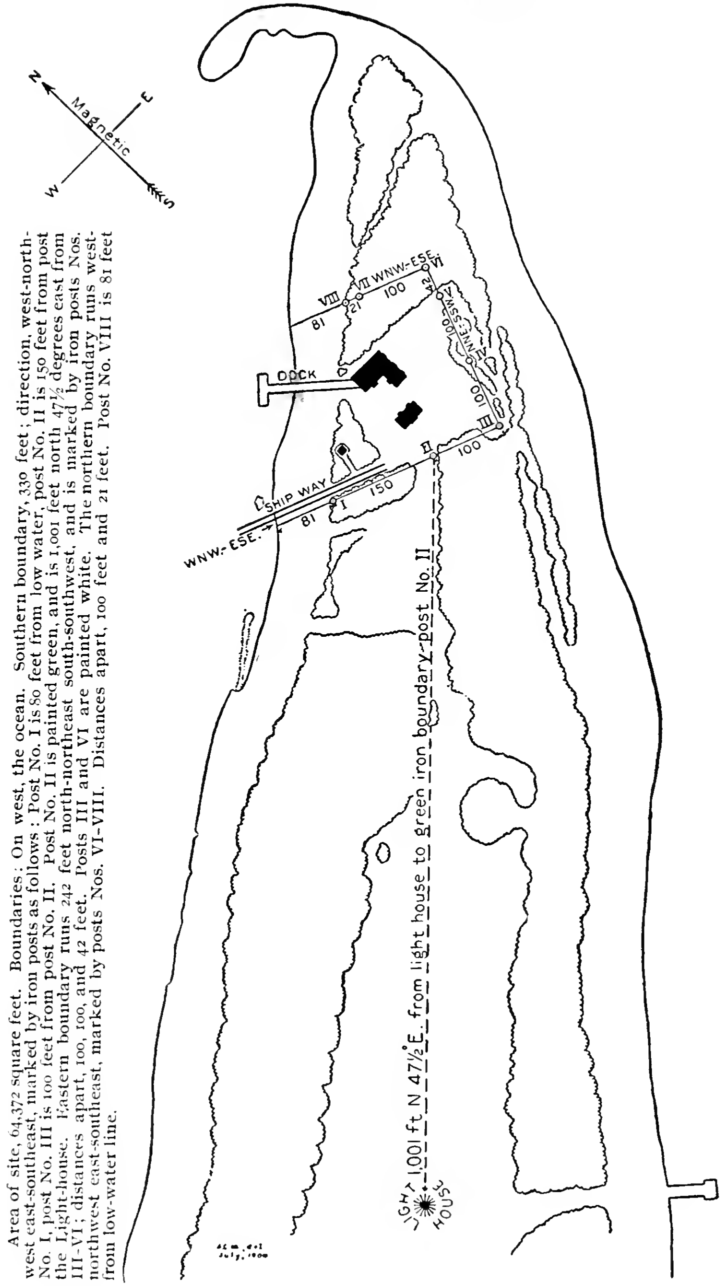



FIG. 4.—Map of North End of Loggerhead Key, Tortugas, Florida, showing site of Carnegie Institution Laboratory.

The vessel was launched at East Boothbay, Me., on August 19, 1904, and completed on August 24. Her cost, including designer's fees, engine, 3,300 feet of Swedish iron dredging rope, winch and friction clutch, plumbing, and one ton of lead ballast, was $6,037.60.

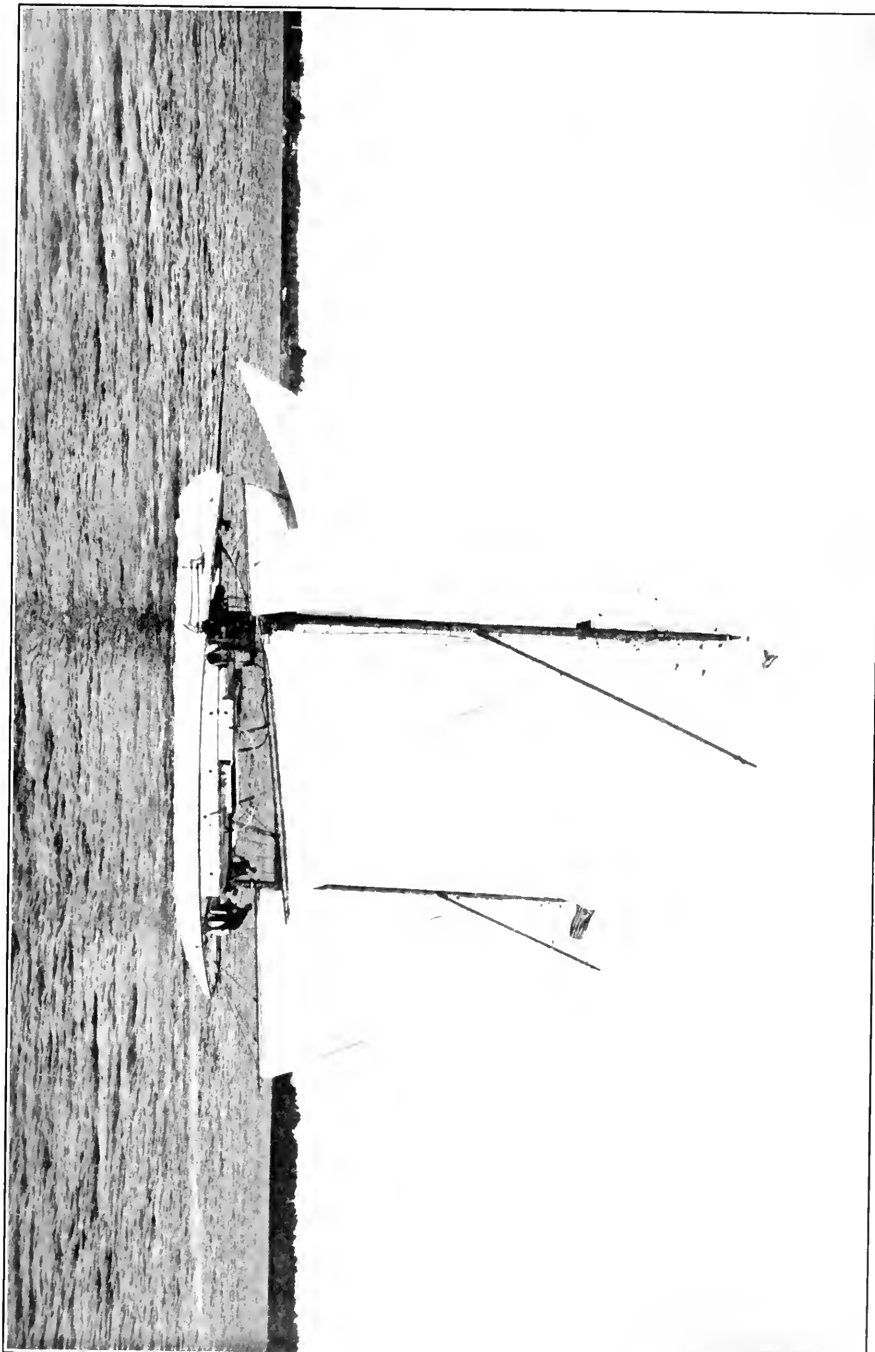
The vessel proves to be one of the ablest yachts of her dimensions on our coast and displays her best qualities in heavy weather. She will make better than 8 points in tacking in a strong breeze, and will either sail or go under power at an 8-knot rate. The gale of September 15, 1904, in which the wind blew at the rate of more than 76 miles an hour, proved her ground tackle to be thoroughly efficient.

The vessel is equipped with a full set of trawls, dredges, deep-sea and surface nets, chemicals, glassware, and apparatus for the study of marine life. She also carries a 15-foot naphtha launch tender, a barometer, sextant, log, U. S. Coast Pilot directions, and a full set of charts of the Atlantic seaboard. Her cabin is designed to provide ample room for such laboratory work as can be accomplished at sea, and in this respect is superior to the majority of vessels of twice her length.

The vessel can best be handled by a crew composed of a sailing master and two men, one of whom serves as cook and steward, the sailing master attending the engine when under power. Under this management the director assumes command of the vessel, taking an active part in her navigation.

The voyage from East Boothbay, Me., to New York was accomplished between August 24 and September 25, more than a week having been spent in Gloucester, Mass., in fitting out the vessel. Many surface hauls were made and some shore collecting accomplished.

The success or failure of the laboratory must depend upon the use made of the excellent facilities which are there afforded. Every possible encouragement must be given to eminent naturalists to pursue their investigations at the laboratory, and their researches must be published in a manner befitting the high aims of the Carnegie Institution.



THE PHYSALIA.

## ECONOMICS.

### REPORT OF DEPARTMENT OF ECONOMICS AND SOCIOLOGY.

BY CARROLL D. WRIGHT.

For the present purposes of the department the following named eleven divisions have been established, and the gentlemen whose names appear have been placed in charge of them, respectively:

Division 1. Population and Immigration.—Prof. Walter F. Willcox, Cornell University, Ithaca, N. Y.

Division 2. Agriculture and Forestry, including Public Domain and Irrigation.—President Kenyon L. Butterfield, Rhode Island College of Agriculture and Mechanic Arts, Kingston, R. I.

Division 3. Mining.—Mr. E. W. Parker, Geological Survey, Washington, D. C.

Division 4. Manufactures.—Hon. S. N. D. North, Census Office, Washington, D. C.

Division 5. Transportation.—Prof. W. Z. Ripley, Newton Centre, Mass.

Division 6. Domestic and Foreign Commerce.—Prof. Emory R. Johnson, University of Pennsylvania, Philadelphia, Pa.

Division 7. Money and Banking.—Prof. Davis R. Dewey, Institute of Technology, Boston, Mass.

Division 8. The Labor Movement.—Carroll D. Wright, 1429 New York avenue, Washington, D. C.

Division 9. Industrial Organization.—Prof. J. W. Jenks, Cornell University, Ithaca, N. Y.

Division 10. Social Legislation, including Provident Institutions, Insurance, Poor Laws, etc.—Prof. Henry W. Farnam, 43 Hillhouse avenue, New Haven, Conn.

Division 11. Federal and State Finance, including Taxation.—Prof. Henry B. Gardner, 54 Stimson avenue, Providence, R. I.

These divisions are actively engaged, except Division 9, in charge of Prof. J. W. Jenks, who since the creation of the department has been in the far East and has only just returned. The progress of their respective activities can best be understood by stating for each the substance of reports which have been made to me.

#### DIVISION 1. POPULATION AND IMMIGRATION.

Prof. Walter F. Willcox, in charge of this division, reports that upon the topic of immigration an index to Niles' Register is being prepared under the immediate supervision of Prof. Davis R. Dewey, and a competent graduate student, who is carefully indexing the material in the library of Cornell University, which library is very rich in the field to be covered. This work is practically completed, and a study of the history of Federal legislation dealing with immigration begun. I may say that the indexing of Niles' Register and other works is being so conducted as to avoid duplication under the different divisions.

A study of the history of Russian immigration is being conducted by M. E. Goldenweiser, of Columbia University, a Russian Jew

of education and ability. His work has been interrupted by the illness of his father, but he will undoubtedly carry it to its completion.

Miss E. G. Balch, instructor in economics in Wellesley College, has undertaken a study of the history of immigration from Austria-Hungary, and of the conditions of the immigrants from that country in certain typical localities in the United States, while Prof. Mary Roberts Smith, although not directly under the Division of Population and Immigration, but working through a research assistantship granted by the Carnegie Institution, is preparing a history of Chinese immigration.

Professor Willcox himself is carrying on an extended study in race and immigration questions that will be very valuable for the work of the Department of Economics and Sociology. I would state, further, that Professor Willcox intends to make the work on population and immigration something more than a statistical statement, dealing largely with sociological results of immigration, and especially, of course, with the economic results of the movement of population, its projection along certain lines of settlement, etc.

### Division 2. Agriculture and Forestry, Including Public Domain and Irrigation.

President Kenyon L. Butterfield, in charge of this division, reports that since assuming the work committed to him he has given most of his thought to perfecting the plan of his investigation and finding men to conduct various phases of his work. Under him, Prof. T. N. Carver, of Harvard University, is studying the economic characteristics of the agricultural industry; Prof. F. W. Blackmar, of the University of Kansas, the economic and social influences of irrigation; while Prof. J. E. Pope, of Columbia, Missouri, is co-operating with the University of Missouri in a history and status of the economic and social relations of the agricultural industry in Missouri.

Mr. A. E. Sheldon, director of the field work of the Nebraska Historical Society, is studying the history of land systems and land policies in the West. Mr. R. H. Leavell, of the Mississippi Agricultural College, is undertaking a study of the race factor in the history and status of agriculture in the Mississippi valley. Mr. Enoch Marvin Banks, of Palmetto, Georgia, is making a research into the tendencies of land ownership in Georgia as revealed in the county tax digests of that State. Mr. Charles S. Potts is also engaged in an intensive study of the history and status of the economic and social relations of the agricultural industry in the Brazos valley,

while others are engaged upon different phases of the economic and sociological aspects of agriculture generally.

### Division 3. Mining.

This division is under the charge of Mr. Edward W. Parker, expert, Geological Survey. Mr. Parker reports that Mr. J. F. McClelland, of the Columbia School of Mines, is in charge of the work on precious metals, and that during the summer he has spent much time in the mining camps of Colorado, and gathered very full data on the history of economic conditions in that State from the time of the first gold excitement. He has also obtained notes of mining in Wyoming. During the winter Mr. McClelland will continue his researches among libraries, and next summer take up more active field work.

Prof. C. K. Leith, of the University of Wisconsin, is in charge of the work on iron ores and the economic influences of mining and working ores. He did a considerable amount of work during the summer in his particular line.

Dr. M. N. Bowles, of the Columbia School of Mines, is in charge of investigations relating to copper. He has already collected much material bearing upon prehistoric copper implements and other matters concerning the mining of copper. His researches have been prosecuted in different parts of the country.

Mr. Walter Renton Ingalls, of New York, is in charge of the investigation relating to lead and zinc. Mr. Ingalls is an acknowledged authority on these subjects, and he has very kindly consented to prepare the work for the economic history. During the summer he did much work in regard to lead-mining industries, acquiring a vast amount of information not previously known. He has in the past few years collected the most complete notes on the history of zinc mining and metallurgy and the uses of the metal, and the Carnegie Institution will have the benefit of the knowledge already obtained.

Mr. W. S. Landis, of Lehigh University, is in charge of some studies relating to chromium and manganese. He has already completed the entire reference work on these two subjects, and the work seems to be in a most satisfactory condition.

Mr. H. H. Stoek, editor of "Mining and Minerals," is in charge of the investigation relating to anthracite coal. During the summer Mr. Stoek was engaged in collecting and arranging a large amount of historical data showing the economic development of this vast industry, and his work is in favorable condition.

Mr. Walter S. Landis, of Lehigh University, is in charge of studies relating to the bituminous coal industry. Mr. Landis has been collecting all material of historical and statistical nature up to 1880, since which time reports of the Federal Government have been available. Mr. Landis is in a position to use the technical collection of the late Eckley B. Coxe, probably the largest collection of books, pamphlets, and reports on coal-mining in the world. On account of this immense amount of material, the work required to cover a given district is very large and progress somewhat slow, but, on the whole, Mr. Landis is of the opinion that his library research, so far as this work is concerned, is about one-third completed.

Mr. G. P. Grimsley, of the West Virginia Geological Survey, is in charge of studies of petroleum and natural gas. He has access to a large amount of original records relating to this subject.

Mr. F. B. Laney, of the University of North Carolina, in charge of inquiry on building stones and quarrying, promises a most interesting chapter on this important subject.

Mr. Heinrich Ries, of Cornell University, is studying the economic influences resulting from the production of clay materials. For a number of years Dr. Ries has been making a study of the clays of this and other countries, and he has altogether in his possession a very large amount of data necessary for this work.

Dr. Joseph Hyde Pratt, of North Carolina, is studying abrasive materials, rare earths, and mica.

Mr. E. C. Eckels, of the U. S. Geological Survey, has undertaken the study of cement, gypsum, and magnesite. He has been able to work up a complete and considerable portion of his data relating to this subject. He will discuss cement materials, and how far they are economically used in the development of building. Mr. Eckels states that other portions of his work are well advanced.

Mr. Ira A. Williams, of the Columbia School of Mines, has undertaken the study of asbestos, barytes, fluor-spar, fullers' earth, talc, graphite, lithograph stone, lithium minerals, mineral pigments, and soapstone. The results of the studies of these various minerals will consist of brief chapters. Mr. Williams has prepared a tentative scheme of treatment, which Mr. Parker has approved.

Prof. Charles E. Munroe, of the George Washington University, Washington, D. C., has taken up chemical materials, and will prepare a report on that subject, but in cooperation with the report on chemical manufactures under the charge of Mr. North.

Mr. Parker reports that he has not yet arranged definitely for the

history of mining legislation. Undoubtedly this work is practically in existence through the history already published by Mr. Curtis Linde, of San Francisco, and probably a condensation of Mr. Linde's work will be ample for the purposes of this department.

### Division 4. Manufactures.

Hon. S. N. D. North, Director of the Census, in charge of this division, was delayed some months in taking up active work, but he reports that substantial and satisfactory progress has resulted from his labors during the past summer. He has secured the cooperation of a number of gentlemen whose qualifications for participating in the work under his charge are of the highest order, and who will come into it with an interest and an enthusiasm essential to the best results.

Prof. W. P. Patterson, of the University of Iowa, is engaged to make a study of the natural resources of the country in their economic relation to manufactures, and of national characteristics.

Prof. G. D. Luetscher, of George School, Pennsylvania, will prepare that portion of the economic history which relates to the economic influence of legislation in the development of American manufactures. This study will cover legislation in both the colonial and the subsequent periods of our history.

Dr. U. B. Phillips, of the University of Wisconsin, in collaboration with Dr. Charles McCarthy, will prepare that section of our history which will deal with the economic influence of slavery on the development of manufactures in the Southern States.

Prof. Henry R. Mussey, of the School of Commerce of New York University, is studying the history of iron and steel manufacture, including both colonial and subsequent periods. He has been at work during the summer, and has entirely completed his researches in respect to the colonial period.

Prof. M. B. Hammond, now of the University of Ohio, has charge of the chapter relating to the history of cotton manufacture. Mr. North considers himself fortunate in securing for Carnegie Institution the services of Professor Hammond.

Other gentlemen will take up specific chapters relating to the economic development of special industries.

### Division 5. Transportation.

Prof. William Z. Ripley, of Harvard University, is in charge of this division. He reports that Dr. U. B. Phillips, of Wisconsin, has

been collecting material and has made personal research on various points. Mr. A. D. Adams, of the Harvard Law School, is studying the early pooling of freight traffic, while Professor Meyer, of Madison, Wisconsin, will arrange the history of railway legislation.

Mr. S. Daggett has nearly completed a study of railway reorganizations, while Dr. T. W. Mitchell, of the University of Pennsylvania, is working on early railroad finance. Prof. A. Pope, of the University of Wisconsin, is engaged on some historical matters concerning the railroads of that State.

Dr. Ripley himself has been working on the history of rate-making systems in the Southern States, which he will follow by a comparison of the history in the trunk-line territory. He has had a number of men working during the summer who have not yet turned in the results of their labors, but he is making satisfactory progress in his division.

### Division 6. Domestic and Foreign Commerce.

Prof. Emory R. Johnson, of the University of Pennsylvania, who is in charge of this division, has been actively engaged personally and through various assistants. He has with him Mr. A. A. Giesecke, of the graduate department of the University of Pennsylvania, who is assisting in the study of the American merchant marine.

The subject of American foreign trade is being ably investigated by Mr. S. Hulbner, assistant in the Department of Commerce of the University of Pennsylvania. This gentleman has collected a large amount of statistical and other data for the period from 1789 to the present time. He will study the colonial period after the national period has been covered.

The history of American coastwise commerce is being studied by Mr. Thomas Conway, jr., a Harrison scholar in the graduate department of the University of Pennsylvania. Mr. Conway has nearly exhausted the printed sources of information for the years since 1789, and is now studying the economic influences of commercial organization as derived from trade journals and other sources of information, original and otherwise. There is a great lack of official statistics in this direction; consequently much must be ascertained from original research.

Dr. J. R. Smith, instructor in commerce at the University of Pennsylvania, has been at work upon the organization and administration of commerce; he has enlarged the scope of his studies somewhat and is to prepare a monograph for our purpose.

The legal and administrative relations of the Federal, State, and local governments in the United States to commerce have been undertaken by Mr. J. B. Byall, of Philadelphia. The work of Dr. Smith and Mr. Byall has nearly covered the fourth subdivision of the general subject of American commerce, which comprises the organization and administrative features.

Mr. Raymond McFarland has prepared an outline of the history of American fisheries. Work is also being done on the American consular service as it relates to commerce.

The Library of Congress is now preparing, at the request of Professor Johnson, a bibliography of American commerce. An effort will be made to have this bibliography as comprehensive as practicable, because it is expected that the work of the Library of Congress will be of assistance to all persons who may work on the history of American commerce.

### Division 7. Money and Banking.

Dr. Davis R. Dewey, of the Massachusetts Institute of Technology, has charge of this division. During the past year he has been engaged chiefly in locating the sources of information which are available for research investigation in the history of banking, and in particular he has endeavored to secure information relative to original sources of information, such as State reports, reports of State banks, etc. He has culled everything, and has arranged all these sources of information for the purpose of showing the commercial growth of banking in different sections of the country. He will deal with credit operations, which must be considered commercially as well as from the institutional standpoint. While some of his tabulations are not to be published in the history, they are essential as a basis of analysis.

Doctor Dewey has had three assistants working on State supervision of banks in Massachusetts, the history of trust companies in Massachusetts, and the history of savings-banks in that State. Dr. Wesley C. Mitchell, of the University of California, is prosecuting an investigation relative to the effects of legal-tender issues on prices and wages between 1865 and 1879, the latter being the date of specie resumption.

Doctor Dewey has also superintended the making of indexes of different works relating to banking, etc., and has noted material for other collaborators in the progress of this work, this being done to avoid duplication.

### Division 8. The Labor Movement.

This division of the Department of Economics and Sociology is under my own charge. Dr. J. H. Hollander, of Johns Hopkins University, with a corps of graduate students, has been for a long time engaged upon the study of all elements or phases of trades unions, including their history, development, constitutions, methods, membership, etc. He has made fine progress with this work, and all the results of his studies are to be available for the economic history of the Carnegie Institution.

A topical analysis of all labor laws of the United States and an analysis of the decisions of courts interpreting them are in process of preparation. These analyses will be so arranged that in a very brief and concrete statement one can learn just exactly what principles of law relative to the relations of employer and employee have been adopted in any State.

Many of the other features coming under this division require principally classification and arrangement, as the information concerning strikes, injunctions, boycotts, employers' liability, the hours of labor, wages, etc., is in existence. The official reports of the Federal and State governments and the investigations of individual students are to be utilized and their results co-ordinated.

Dr. Richard T. Ely, of the University of Wisconsin, has projected quite an ambitious work on industrial democracy, in the preparation of which he will make various original studies. I have arranged with Dr. Ely for an exchange of data in order to avoid the expense attending duplication of research.

### Division 9. Industrial Organization.

Dr. J. W. Jenks, of Cornell University, is in charge of this division, but, as already explained, on account of his absence in China for the Federal Government, he has not entered actively upon the discharge of his duties.

### Division 10. Social Legislation, Including Provident Institutions, Insurance, Poor Laws, etc.

This division is under the leadership of Prof. Henry W. Farnam, of the Sheffield School, New Haven. Professor Farnam has made considerable progress in his work, and has had under his employment several assistants, among whom is Mr. F. R. Fairchild, who has completed a study of the factory legislation of New York. Mr. George C. Groat, of Columbia University, is at present at work upon that phase of the social legislation of the State of New York

which relates mainly to labor organizations and trade disputes. Professor Farnam hopes to organize the work of his division on a more extended scale during the autumn.

DIVISION II. FEDERAL AND STATE FINANCE, INCLUDING TAXATION.

Dr. Henry B. Gardner, of Brown University, very kindly undertook this work. He finds that while the subject of national finance has been gone over several times and the outlines of the subject have been fairly clear and the sources of information practically well known, yet nothing has been done in the field of State and local finance since 1879. His first work, therefore, was to project a study into the financial history of the individual States and typical cities, and he has interested graduate students or instructors in this direction. Several gentlemen have already undertaken to do work, among them Mr. Frederick A. Wood, of Vermont, the author of "The History of Taxation in Vermont"; Prof. C. H. Brough, Ph. D., now of the University of Arkansas, who is the author of an essay on "Taxation in Mississippi," and Prof. St. George L. Sioussat, of the University of the South, who will deal with taxation in Tennessee.

Prof. E. L. Bogart, of Oberlin College, has been engaged during the past year in the study of the financial history of Ohio, and he will continue this work. Prof. W. A. Rawles, of the University of Indiana, will conduct the researches for that State. Prof. W. O. Hedrick, of the State Agricultural College of Michigan, is engaged in the study of special taxation in that State, under the direction of Prof. Henry C. Adams, of the University of Michigan.

Minnesota, Kansas, South Dakota, and California are under way. California will be treated by Prof. C. C. Plehn, of the University of California, one of the best-known authorities on the subjects treated in this division. Correspondence is going on with gentlemen in other States, and they will probably enter upon work under the direction of Professor Gardner.

Professor Gardner has engaged Mr. William Jones, of Brown University, who will undertake much of the work of investigation committed to Professor Gardner. Professor Gardner has also undertaken a card bibliography of financial publications, covering not merely the items of interest in his own work, but those which bear upon other divisions as well.

In general, Professor Gardner will discuss conditions in 1789, including an account of both State and local finance; receipts and

expenditures of States since that year; a chronological account of legislation, including constitutional provisions and judicial decisions for the same period; the relation between the finances of the State and the political system; and general economic conditions. He will also discuss the working of the more important forms of taxation, such as general property tax, taxes on banks and insurance companies, taxes on railways, corporation taxes, inheritance taxes, income taxes, business taxes, etc. In addition, he will enter upon a study of the financial aspects of internal improvements, and give a history of State debt and credit, and an account of the development of financial administration, including budgetary practice.

I may say in general that every effort is being made to co-ordinate and harmonize the work of divisions whose subjects interlock; as, for instance, there are several features under manufactures, transportation, and domestic and foreign commerce that offer opportunities for conflict, but the gentlemen in charge of these divisions are working thoroughly in harmony, and will see to it that no complications arise. This is true of the divisions relating to money and banking and Federal and State finance. Professors Dewey and Gardner are working together, so that there shall be no duplicate treatment of subjects. All these gentlemen are looking carefully to the fact that when one is collecting information along certain lines it may be desirable to enter information for another. This is true also of the divisions relating to the labor movement and to social legislation and industrial organization.

A committee of three, consisting of Messrs. North, Gardner, and Dewey, has been appointed to consider and report upon a plan for a useful bibliography of economic history. All realize that the ordinary bibliography should not be constructed, but one that will be of positive use on a most advanced plan to all concerned.

I am greatly gratified at the progress of the work of the whole department as shown by the preceding statements, which consist of brief condensations of the reports of the respective collaborators. I have every confidence in the work as it is being conducted. If the work of the first six or eight months has developed nothing more than concrete and workable plans, without very much progress, we should be satisfied; but it has gone farther than that, and while much remains to be done in the way of formulation of methods of procedure and their co-ordination into one general plan, nevertheless there is no doubt now of the success of the work committed to the Department of Economics and Sociology.

## HISTORICAL RESEARCH.

By Andrew C. McLaughlin, Director.

The work of the Bureau of Historical Research during the past year has been of various kinds. Considerable time has been consumed in assisting or giving suggestions to historical investigators who have come to Washington in search of material for their work. In a few cases documents have been hunted out and copied for the use of those who were unable to come to discover the material for themselves. The experience of the year seems to prove that, while this incidental work does not give at first very tangible results, it is of considerable value and justifies in itself the existence of the Bureau in Washington.

At the beginning of the year the hope was entertained that the Guide to the Archives in Washington would soon be completed, but the work was not entirely finished until the first of October. The completed volume, bearing the title, "Guide to the Archives of the Government of the United States at Washington," is a book of over 200 pages. It describes in general terms the historical collections and the administrative records of all branches and departments of the government. Practically every bureau, commission, or office having its independent records receives attention; its duties are indicated, and the character of its records briefly described. This work was begun in January, 1903, by Mr. C. H. Van Tyne and Mr. W. G. Leland, and was carried to completion by the Bureau, most of the work after October, 1903, being done by Mr. Leland. Though necessarily condensed, for the book purports to be only a guide based on a general survey, it represents much labor, for often the acquiring of accurate information, which was in the end told in a few words on the printed page, required days of patient looking and questioning. The guide will help the historical investigator to know where to look for his materials, will in many instances let him know whether he can reasonably expect to find the materials he seeks, and will, moreover, furnish the necessary basis for further study of the historical records of the government.

Prof. Charles M. Andrews, of Bryn Mawr, has made for the Bureau an examination of the British archives, and has prepared a preliminary report on the character, extent, and location of the materials for the study of American history. This report will soon be printed, probably in the American Historical Review, and will serve admirably as a basis for more extended as well as a more particular examina-

tion. With a little more work, Professor Andrews can prepare a general and comprehensive survey of the sources of American history in the Public Record Office and all the other important places of deposit in Great Britain. Steps have also been taken to gather information concerning such transcripts from English archives as are now in the libraries of this country, and through the kindness and courtesy of the American Antiquarian Society a list of documents in English archives that are now in print and throw light on American history has been turned over to this Bureau for its use. This list was prepared some three years ago and will need to be brought down to date. When all of these tasks, which are now under way, are completed, the Bureau will have in its possession and ready to print material for a volume showing the character, extent, and place of deposit of the sources of American history in the public depositories of Great Britain, of the transcripts of those sources that are accessible in this country, and of the documents that are now in print.

It is plain from what has already been said that the activities of the Bureau have been confined to tasks which, when completed, will enable the historical investigator to reach and use his materials more easily. In carrying out this general idea, it has seemed wise to begin the preparation of a bibliography of current writings on American history. The list for the year 1903 has been prepared and will soon be ready for the press. It includes altogether not far from 4,000 titles. In addition to the ordinary bibliographical entries, references are made to the most helpful published reviews of the listed books, and with the title of each important book is given in a few words a description of the book, showing its scope and general character. Under the supervision of the director of the Bureau, this work has been carried on chiefly by Mr. William Adams Slade and Miss Laura Thompson, both of the Library of Congress.

The work of discovering letters sent to State governments by the delegates to the Continental Congress has been begun and some progress has been made. This undertaking requires considerable time and effort, and it is not likely that the work can be accomplished even by the end of next year.

Various bodies of archives have received particular attention, in order that the Bureau may be able to answer questions as to place and character of certain kinds of historical material. The diplomatic correspondence in the Bureau of Indexes and Archives of the State Department for the first fifty years of our history under the Constitution has been examined page by page, although, of course,

not all has been read or particularly classified. It is the intention of the Bureau to prepare before the end of the current year a full report on the nature, extent, and condition of these papers, to give a close estimate of the proportion already printed in the "American State Papers," and to indicate the nature of the important historical information they contain, especially in periods of peculiar interest. A large portion of this task has already been accomplished. A few documents of special importance have been discovered and edited, notably a sketch of "Pinckney's Plan for a Constitution, 1787," printed in the "American Historical Review," July, 1904.

The beginning of what it is hoped may be a valuable series of monographs has been made by the publication of "The Influence of Grenville on Pitt's Foreign Policy, 1787–1798," by Prof. E. D. Adams, of Stanford University.

The task of making a full list of the Washington letters has been begun. While there are many of these letters in a few collections, others are widely scattered throughout this country and Europe, and the preparation of anything approximating a complete list will naturally be the work of some years.

## TERRESTRIAL MAGNETISM.

The subject of an international magnetic bureau is fully presented by Dr. L. A. Bauer in Year Book No. 2, accompanying papers, pp. 203–212. The Executive Committee recommended to the Board of Trustees that a grant of $20,000 be made for magnetic research by the Carnegie Institution, it being proposed not to take up such magnetic work as is already well provided for by national bureaus, but only such as lies outside the proper sphere of activity of these bureaus, the nature of whose appropriations usually limit their work within the confines of their countries. Furthermore, the purpose is to gather together and unite in one harmonious whole all existing knowledge and facts, so that the directions in which future work can most profitably be accomplished will be set forth. The investigations promise not only to have scientific utility, but to reach results of great practical importance, *e. g.*, the determination of the magnetic data necessary for safe navigation at sea.

The favorable action of the Trustees at the annual meeting in December, 1903, and the reference of the project to the Executive Committee resulted in the formation of a Department of International Research in Terrestrial Magnetism, with Dr. L. A. Bauer as director, and with authorization to begin work April 1, 1904. The first allotment was $20,000.

### Report of the Department of International Research in Terrestrial Magnetism.

By L. A. Bauer.

In conformity with the authority conveyed in the Secretary's letter of March 29, 1904, the work of the above department was begun on April 1, 1904, and since then has been steadily prosecuted.

The foreign advisory council consists at present of the following members: Professors J. Elster and H. Geitel, of Wolfenbüttel, Germany (advisers in atmospheric electricity); Prof. E. Mascart, Director of the Bureau Central Météorologique of France; Prof. A. Schuster, Director of the Physical Laboratory, Owens College, Manchester, England; Prof. Adolf Schmidt, in charge of the Potsdam Magnetic Observatory, Germany.

Owing to the large amount of office work that could at once be taken up with the force available, it proved advantageous on account of the conditions under which some of those employed could

render service, to rent modest private quarters in addition to those furnished in the Coast and Geodetic Survey Office. Such other requisite facilities as were possible were readily and courteously furnished by the Superintendent of the Coast and Geodetic Survey for the furtherance of the work of the department; thus instruments and books were loaned, and training in observation and computing was given to certain employees.

## OFFICE WORK PERFORMED.

INVESTIGATION I. A general compilation and discussion of magnetic data for the complete presentation of our existing knowledge of the secular variation of the earth's magnetism over the entire globe, with the view of determining the points at which it will be necessary to repeat observations at suitable intervals, for the successful scientific investigation and determination of the causes and their modes of action, and for ascertaining the proper corrections to magnetic charts to refer them to a desired date.

This investigation is in progress and will require some time for completion. It involves a number of initial, related researches for furnishing the necessary data and methods so as to permit exhibiting and publishing the results on a consistent and homogeneous basis. Thus, frequently a critical study of the observer's methods and instruments must be made in order to furnish clues for the interpretation of discrepancies either between his own results or between his and those of another observer at the same station. So also it has been found necessary to make a critical study of the existing magnetic maps since those of Sabine for 1840-45, with the view of exhibiting the state of our existing knowledge of the distribution of the magnetic forces and of the secular changes. Likewise, in order to furnish the necessary reduction corrections to the observed quantities, it was requisite to make a compilation of data pertaining to the diurnal variation of the magnetic elements and to determine the laws governing their geographical distribution.

From these correlated studies useful permanent information has been obtained and certain interesting and important results deduced, of which the chief ones are:

> Comparatively little increase in our knowledge of the general distribution of the earth's magnetic forces has been made during the past half-century, in consequence of which certain constants requisite for the theory of the earth's magnetism are not known at present with any greater degree of certainty than for the epoch of the construction of Sabine's charts (1840–45).

In spite of the apparently vast accumulation of data, such an important question as whether the earth's magnetic energy is increasing or decreasing and the annual rate of change can not be definitely answered. The chief reason for this unfortunate state of affairs is that the accumulated material has not the required general distribution, but pertains chiefly to civilized and restricted land areas, leaving almost neglected the greater part of the earth covered by water. Systematic magnetic surveys of the oceanic areas are entirely lacking, such results as are at hand having been obtained from occasional expeditions or incidentally to other work. There is here revealed to the Institution a most useful and promising field of work, and definite recommendations pertaining to this matter will be given in a separate communication.

The completion of the critical study of the modern magnetic charts furnished the necessary data for drawing the following conclusion of great interest in terrestrial magnetism, atmospheric electricity, and meteorology, viz :

> All of the modern magnetic charts—*i. e.*, since those of Sabine for 1840–45—unite in indicating the probable existence of vertical earth-air electric currents of the average intensity over the region 45° N. to 45° S. of $\frac{1}{30}$ of an ampere per square kilometer of surface. These currents of positive electricity proceed upward (from the earth into the air) near the equatorial regions, where there are ascending air currents, and downward near the parallels 25° to 30°—*i. e.*, in the regions of descending air currents. Near the parallels 40° the electric currents are again upward, thus corresponding once more with the general atmospheric circulation. Beyond the parallels 45° the results appear too uncertain to warrant drawing a definite conclusion.

In order to make some tests as to the manner of distribution of the upward and downward electric currents, the currents over quadrilaterals bounded by two parallels 10° apart and two meridians, likewise 10° apart, have been derived for the entire region from 60° N. to 60° S. for the three epochs 1842, 1880, and 1885. As a general result it did not appear as though the directions of the electric currents—whether up or down—were to be associated with the distribution of land and water. There was, however, a decided indication, *for each epoch*, that over the areas of low pressure, where the air currents are upward, there the electric currents were likewise, in general, upward, and that over the areas of high pressure, where there are descending air currents, there the electric currents were likewise descending.

Thus, as the average result from the three epochs we have :

| Region. | | Quantity of electricity. |
|---|---|---|
| 60° N. to 60° S.. | For areas of low pressure ........... | $+829 \times 10^4$ amperes. |
| | For areas of high pressure .......... | $-638 \times 10^4$ amperes. |

(+ means upward electric currents ; — means downward electric currents.)

The average effect of electric currents for the region 45° N. to 45° S. is on the east-west component of the earth's magnetic force, 0.001 C. G. S. unit, or about one-fiftieth of the average value of this component. The average effect on the horizontal intensity is about one one-thousandth part—*i. e.*, on the order of the error of a field determination. However, the average effect on the declination is about 0.2°, about six times the error of a reduced field determination of the declination on land and about one to two times the error of a determination at sea by the most approved methods.

Another result of prime interest to the magnetist, geologist, and geophysicist was deduced with the aid of the existing magnetic charts. About 65 to 70 per cent of the total magnetization of the earth can be referred to a uniform or homogeneous magnetization about a diameter inclined 11.4° to the axis of rotation. Deducting this "primary" portion, there is left a "secondary or residual field," representing the want of uniformity in the distribution of the earth's magnetism. This secondary field has been mapped out by the department for the two epochs 1840–45 and 1880, the writer having mapped out, in 1896 and 1899, in a similar manner, this field for 1885. The same general characteristics are exhibited for the three epochs.

> It is definitely shown that the residual permanent magnetic field of the earth is not a heterogeneous one, such as it would be if, for example, its formation were primarily to be referred to the irregular distribution of magnetic materials in the earth's crust. On the contrary, although the magnetic system is somewhat complex, it is yet quite systematic in its structure, consisting chiefly of two main magnetizations approximately transverse to the axis of rotation. There is, therefore, a very strong indication that this field is produced by some distinct physical cause operating in the same general manner over the entire earth. The hope is thus clearly held out that we may still further resolve the residual field, starting with fundamental, physical causes. The present belief is that the chief physical cause of the residual field is to be referred to the distribution of temperature within the stratum of the earth's crust here concerned.

For there is a very remarkable correspondence between the principal features of the residual magnetic field and those exhibited on

a chart of isabnormal temperatures. It was found that the earth as a magnet acts like any other magnet as regards application of heat. Thus, wherever the earth's surface is relatively warm, on the average for the year, there the magnetization of the earth shows a decrease, and where, on the other hand, it is relatively cold, there it suffers an increase.

It was further found that—

> there is very close similarity between the residual permanent magnetic field of the earth and that of the field of forces causing the diurnal variation of the earth's magnetism; and there appears to be more than a mere chance connection in this relation, as is shown by the simultaneous studies of the vector diagrams for various parallels as resulting from the two respective fields.

INVESTIGATION II. Discussion and publication of the data on the magnetic perturbations observed during the eruption of Mont Pelée, Martinique, 1902.

The data obtained as the result of a circular letter sent by the Superintendent of the U. S. Coast and Geodetic Survey to observatories over the entire globe were turned over by him, as agreed, to the department for discussion and publication. First, the investigation of the notable magnetic disturbance coincident with the eruption on May 8, 1902, was undertaken, the necessary information having been extracted from the reports and magnetograms received from twenty-six institutions distributed over the globe, and most important results have been derived.

> It was found that the Mont Pelée magnetic disturbance of May 8, 1902, resembled a cosmic one in two respects, viz: First, that the time of beginning of the disturbance was practically the same around the whole earth; and, second, that any electric-current system capable of producing the observed phenomena would have its seat chiefly outside the earth.

Owing to the peculiar vaporous nature of the products of the eruption, it would appear as though their violent ejection was accompanied by the formation of electric charges above the earth's surface sufficient to disturb the entire potential of the earth. We thus have had shown us how a magnetic storm can be produced by a tremendous explosion, and the further study may throw some light upon the connection between terrestrial magnetic storms and solar eruptions, and on the *modus operandi* of the operating forces. The investigation is therefore being continued so as to include other disturbances occurring at about the same time, and an examination

will be made of any possible previous instances in which terrestrial eruptions were accompanied by magnetic disturbances.

> The average time of the beginning of the magnetic disturbance on May 8, 1902, was $7^h\ 54.1^m$ a. m., St. Pierre local mean time. According to Heilprin, the hands of the clock on the town hospital were found stopped at $7^h\ 52^m$.

As it is not known how accurately the clock kept local mean time, it is probable that the time as given by the magnetic disturbance is the most accurate determination of the time of the eruption to be had.

INVESTIGATION III. Compilation, discussion, and publication of the existing data concerning the variations of the earth's magnetism other than the secular variation already provided for in Investigation I, and the perturbations of the earth's magnetism.

In connection with Investigation I, it has been found necessary, as related in that section, to make some preliminary studies on the geographical distribution of the corrections due to the diurnal variation of the magnetic elements. More than this it has not been deemed wise to attempt at present, until the correspondence with institutions and persons has been completed. Some preliminary negotiations have been entered into with Prof. Adolf Schmidt, in charge of the magnetic observatory at Potsdam, Germany, regarding the discussion of recent magnetic storms, to be conducted under his direction, with the aid of computers employed by the department.

MISCELLANEOUS.

In order that the department may have full knowledge of material and investigations, so as to avoid duplication and reveal deficiencies, a circular has been prepared for forwarding to persons and institutions engaged in work relating to the department. A card catalogue is furthermore being kept of all publications and data obtained, for ready reference by the members working in the department and for rapidly replying to calls for information from outside. The department is thus enabled to fill an important need in magnetic research.

The department was represented by the director at the following congresses, viz: Eighth International Geographic Congress, International Electrical Congress (St. Louis), and International Congress of Science and Arts (St. Louis). At each of these congresses he presented, upon special invitation, papers relating to the earth's

magnetism. He was also a delegate to the International Solar Research Conference meeting in St. Louis, September 22.

The director has also been appointed a member of a committee of the International Association of Academies, which is considering methods for securing increased accuracy in magnetic work at sea.

## FIELD WORK.

Nothing further could thus far be attempted under this head than to place orders, as authorized, for instruments required in future work, study various designs, train certain of the employees in field work with the aid of the facilities furnished and instruments loaned by the Superintendent of the Coast and Geodetic Survey, and to test some recently arrived instruments. Preparations are being made for international coöperation in magnetic and allied observations during the solar eclipse of August 29–30, 1905, and a circular has been issued inviting the cooperation of all those who can take part in this important work.

Plans for systematic magnetic surveys of the oceanic areas have been carefully considered. One magnetic outfit required for such work has been received from the maker, and the constants of the instrument have been determined. Also a feasible plan for a rapid, systematic magnetic survey of the North Pacific Ocean has been worked out, in collaboration with Mr. G. W. Littlehales, hydrographic engineer of the U. S. Hydrographic Office, and with the advice of Captain E. W. Creak, formerly Superintendent of the Compass Department of the British Admiralty, now retired, and of O. H. Tittmann, Superintendent of the U. S. Coast and Geodetic Survey. This project is published in full elsewhere.

## SPECIAL GRANTS.

### TRANS-CASPIAN ARCHEOLOGICAL EXPEDITION.

(Raphael Pumpelly, Newport, R. I., in charge. $18,000.)

In Year Book No. 2, pages 271–287, there is a brief report of Prof. Raphael Pumpelly's first expedition to the Trans-Caspian region. The second expedition was for the purpose of archeological investigations in special areas noted on the first expedition. The following report is an indication of the character of the results obtained. The final report will be prepared as soon as practicable.

Professor Pumpelly left America in December, 1903. A week was passed in Berlin, where he engaged as archeologist Dr. Hubert Schmidt, of the Museum für Völkerkunde. Dr. Schmidt had excavated at Troy under Dörpfeld, and is an expert in prehistoric pottery. A month was passed in St. Petersburg in getting permission to excavate in Turkestan.

On the 24th of March work was begun at Anau, near Askhabad. The members of the party were Dr. Hubert Schmidt, archeologist; Ellsworth Huntington, R. W. Pumpelly; Langdon Warner, Hildegard Brooks, Homer Kidder, volunteer assistants.

Professor Pumpelly chose Anau for beginning because in 1903 he had seen in a cut in one of the tumuli painted hand-made pottery and an abundance of bones. Its structure convinced him that it had been a site of very ancient and long-continued occupation, and he hoped that its bones might throw some light on the source of our domestic animals.

The excavations in these tumuli and several shafts sunk in the city of Anau traversed over 170 feet of the accumulations of successive generations of peoples and extended from recent times down through the iron and bronze civilizations 45 feet deep into the stone age. One tumulus, with now 60 feet of accumulation, was abandoned before the other was begun, and this younger one grew to a height of over 70 feet, after which the neighboring city was founded, and has now about 38 feet of accumulation. The time gaps between the two tumuli and between the younger one and the city are, of course, unknown quantities.

In the northern older tumulus the pottery is all hand-made, much of it with painted decorations; the lower 45 feet of culture-strata (or earth and refuse residuum of long-continued occupation) shows a culture with little or no knowledge of metals. Knives and domestic

implements of flint abound, but no arrowheads or indeed any weapon of offense or of the chase was found in the lower division. In the upper 15 feet there appear remains of objects of copper and lead. Copper without a trace of tin is shown in the only analysis made as yet; other analyses will follow.

These two divisions are also sharply distinguished by a change in the technique and painted decoration of the interesting pottery.

The southern younger tumulus shows also two culture periods. Its founders were already acquainted with the potter's wheel, and all the pottery was made on the wheel. There was little painted ware, and that was of inferior decoration. Of the 75 feet thickness of culture-strata, the lower 63 feet show a fully developed bronze culture. The upper division, 13 feet thick, is marked by the presence of iron objects and by a well-defined change in the character and technique of the pottery, and, further, in the burial customs. A peculiar form of burial existed through both of the culture periods of the older tumulus and through the bronze period of the younger tumulus—burial in a "contracted" position under the floors of the dwellings. The twenty-eight skeletons studied by Mr. Warner were of very short stature; whether of children or of adults remains to be determined by a study of the skeletons. This custom seems to have stopped with the advent of the iron culture.

Professor Pumpelly suspected in 1903 that these tumuli were older than the present surface of the surrounding plain. The excavations of the present year show that their bases stand buried, respectively, at least 27 feet and 23 feet deep in the younger strata of the plain.

In order to study the relation between the progress of natural events and the growth of these tumuli and their cultures, numerous shafts were sunk both in the plain and to the bottom of the tumuli and of the city, and Mr. R. W. Pumpelly made surveys and studies bearing on the local physiography in relation to the archeology.

It was found that of the 27 feet of growth of the plain the lower 12 feet were due to natural river sediments and the upper 15 feet to irrigation sediments; but a surprising result of the study is the proof that this whole growth was a relatively late episode in the life of the tumuli. Only a brief outline of the history can be given here.

The streams that rise in the high mountains of northern Persia in emerging onto the Turkoman plains spread out and lose their velocity and deposit their silt, forming fan-shaped deltas, covering many square miles, and each making an oasis. The water is now

all used for irrigating these fertile spots. Beyond them is the desert. Anau is on one of these fans.

The history of these tumuli and of the city is sharply characterized by the following four periods in the history of the plain or subaerial delta:

(1) The north tumulus when founded stood on a hill at least 7 feet, and probably more, above the general plain surface, its dwellings spreading down the slopes. The plain was then increasing its height, through the deposition of river sediments, and continued to grow until it had buried the base of the tumulus to a depth of 2 feet. By that time, or later, the north tumulus was abandoned and the south tumulus founded on an elevation about 2 feet above the plain. The plain continued to grow until it had buried the base of the south tumulus to a depth of 14 feet.

(2) Then followed a change of conditions, either climatic or orogenic. The plain was cut down at least 19 feet.

(3) This was followed by another change, which caused the refilling of the cutting to the amount of 8 feet, *7 feet of this last growth having occurred after the deposition in its sediments of pieces of the thoroughly characteristic pottery of the youngest (the iron) culture of the south tumulus.*

(4) After this, irrigation began, through which the surface of the plain was raised 15 feet higher, bringing it to its present condition, in which the north tumulus stands embedded to a depth of 27 feet, the south tumulus 22 feet, and Anau city 15 feet.

Thus it is evident that the whole of this growth has taken place since the topmost 13 feet of the youngest tumulus was started—*i. e.*, after the accumulation of the 123 feet of bronze and neolithic culture-strata. The base of the 38 feet of culture-strata under the city of Anau stands on the same level as the base of the 15 feet of irrigation sediment that surrounds it. The whole of this 15 feet of irrigation deposit has, therefore, grown since the founding of Anau.

The maximum thickness of irrigation deposit in the oasis is apparently 22 feet. It was shown above that 15 feet of irrigation material and 7 feet of natural sediment had grown up since some time after the introduction of iron. Our observations show that the growth of natural sediments was much slower than that of irrigation material. Indeed, irrigation retains on the fields all of the silt which would otherwise flow beyond the oasis. Therefore there can be little doubt that irrigation in this region was introduced during the iron stage of culture.

The observations made have established approximately the relative ages and rates of growth among themselves of the natural sediments, the irrigation deposits, and the culture-strata. It remains to correlate either of these with a chronological date. Unfortunately, the coins thus far discovered were all of copper alloy and altered beyond legibility, and the dating value of the various objects found will be known only after further study by specialists.

If the work should be continued, Professor Pumpelly has little doubt that the culture-strata of the city of Anau will supply the material needed to complete a most valuable time-scale.

The objects collected at Anau fall into four categories:

(1) A large amount of pottery most systematically collected by Dr. Schmidt and studied by him at St. Petersburg.

(2) Five hundred and ninety-eight numbers of special objects, representing all the objects used in daily life except the pottery and larger stone implements. These also are being studied at St. Petersburg by Dr. Schmidt.

(3) Large stone implements.

(4) Many hundred pounds of bones of animals which were systematically collected at the older tumulus. These have been studied by the archeological osteologist, Dr. Dürst, at Zurich. A recent report from him shows that in the beginning of the oldest culture zone of the tumulus—*i. e.*, in the lower fifth part, there were only wild animals, as follows:

Wild ox, *Bos namadicus* Falconer, agreeing closely with *Bos namadicus* of the Central Asiatic Pleistocene, which represents for Asia the *Bos primigenius* Boj.

Wild sheep, *Ovis arkal* Blasius.

Wild boar, *Sus scrofa ferus* Gmelin.

Gazelle, *Gazella subgutterosa* Güldenstaedt.

Fox, *Vulpes montanus* Pearson.

Wolf, *Canis lupus*.

The horse appeared to be *Equus caballus* L. (*fossilis robustus* Nehring), agreeing remarkably with *Equus caballus* of the European diluvial. Dr. Dürst is not sure that the horse was not tamed. The progress of domestication of the ox and sheep is clearly shown and begins to appear at about 12 feet from the bottom. From the wild *Bos namadicus* (*primigenius*) were developed the domestic cattle, at first as large as their ancestors, but diminishing to a smaller size in the layers of the upper or copper (or bronze) culture of the tumulus.

Equally clearly defined is the gradual progress of evolution from

the long-horned wild sheep, *Ovis arkal* Blasius, through the domesticated contemporary long and short horned animals, of which one form stands very close to the *Ovis palustris* of European culture-strata, and with occasional hornless individuals in the upper layers of the lower culture, to marked frequence of hornless sheep in the upper or copper culture.

The goat appears to have been imported already domesticated from Iran, as it corresponds to the wild forms of that region and the Caucasus.

While only the wild boar, *Sus scrofa ferus* Gmelin, occurs in the oldest culture-strata, there comes in at about 12 feet above the bottom a much smaller pig, corresponding to *Sus palustris* of the lake dwellings of Europe and probably derived from the neighborhood of India.

The camel, *Camelus bactrianus*, does not appear till in the upper or copper culture of this tumulus.

In the great collection of bones from this tumulus there is no trace of the domestic dog, the cat, the ass, or of fowls.

Dr. Dürst's is the most important contribution made as yet in connection with the relation of European culture to Asiatic migrations, being based, as it is, on material from excavations.

On the 18th of May the expedition left Anau for Merv, practically driven away by the vast quantity of decaying locusts in our pits and on the fields.

At Old Merv only two weeks were spent, with about 150 workmen, in reconnaissance excavating to decide as to the desirability of extended work and the nature of the problem. This work was confined to the ruins of Giaour Kala, a city of several square miles area and up to 50 feet thickness of culture-strata. The effects of the intense heat and of enteric disorders, both on the natives and on the members of the party, cut the work short. The results will appear only after the study of the finds, now being made by Dr. Schmidt.

In judging what has been accomplished during the past short season's work, it should be remembered that Russian Central Asia is an absolutely new field, archeologically speaking; there have been heretofore practically no scientific excavations, the excellent investigations of the Russian archeologists having been confined to Russia proper, Siberia, and the Caucasus. Professor Pumpelly had therefore practically no clews to follow other than those furnished by his observations of 1903 over a large area and necessarily of a superficial character.

## GEOPHYSICAL RESEARCH.

(For experiments on elasticity and plasticity of solids. George F. Becker, Washington, D. C. Grant No. 172. $7,500.)

The space for these experiments, which was furnished by the U. S. Geological Survey, became available in July. A testing machine, built to order by Riehlé Brothers, and other apparatus has been installed and various preliminary tests have been made. Mr. Taft, Secretary of War, in recognition of the importance of the investigation, has consented to allow the Washington Monument to be employed for experiments on the elongation of wires under varying loads. A vertical air-tight tube has been put in place from the top to the bottom of the stairway, and observations will begin soon. Wires nearly 500 feet in length will be annealed in the vertical tube by electricity and their elastic elongations determined to a minute fraction of a millimeter by Mr. J. R. Benton.

It has been shown by Dr. Becker that there is extremely strong theoretical ground for the belief that the load-strain function is logarithmic, and his assistant, Mr. C. E. Van Orstrand, has since reached the same result by an independent method. Experiments by Dr. Becker on india-rubber, carried as far as strains doubling or halving the length of cylinders, have been shown to agree with this law. The experiments of Mr. J. O. Thompson, made some years since in Kohlrausch's laboratory, on steel, copper, and silver wires, have been computed by Mr. Van Orstrand. They, too, agree minutely with the logarithmic law.

It is believed that the equipment will be completed by November 1.

(Investigation of mineral fusion and solution under pressure. Arthur L. Day, Washington, D. C. Grant No. 171. $12,500.)

The general purpose of the grant was to increase and extend the work of the high-temperature research in certain particular directions: (1) By increasing the scope of the researches of the rock-forming minerals at extreme temperatures; (2) by providing for experimentation at extreme pressures as well; and thereby (3) to develop apparatus for experiments upon aqueo-igneous fusion.

The grant was made upon condition that suitable laboratory space be set apart for the purpose in the U. S. Geological Survey. The space provided became available on July 1, and has since been equipped by the Survey with the usual laboratory facilities, power for an instrument shop, and electrical connections of good size and variety.

Plans have been prepared for the following apparatus, a part of which is being built in the laboratory shop and part elsewhere. The work of construction is already well advanced.

(1) An apparatus for the fundamental investigation of temperature values above 1,200° C.

(2) An electric furnace of the graphite resistance type for generating extremely high temperatures under moderate gas pressures.

(3) A platinum resistance furnace, in which extreme pressures are developed under moderately high temperatures.

(4) An iridium resistance furnace (Nernst model), in which temperatures up to 2,000° can be reached in a neutral atmosphere or vacuum.

(5) An electric plant and regulating facilities for supplying proper current to these furnaces.

(6) Suitable apparatus for developing the pressures which will be required for the investigations.

The following researches will be begun as soon as the apparatus is ready:

(1) A fundamental investigation of temperature measurement above 1,200° C.

(2) An investigation of fusion and solution phenomena in certain feldspars and pyroxenes.

(3) The development of apparatus for the simultaneous application of pressure and temperature to the rock-forming minerals in the presence of water.

The second investigation is already well under way.

(Preparation of a bibliography of geophysics, requiring two years. Carlos de Mello, Washington, D. C. Grant No. 170. $5,000.)

The period of Mr. de Mello's work covers nine months, beginning January 1, 1904. The work is being carried forward under twelve subjects, as follows:

1. General and synthetical works on dynamical and structural geology, physical geology, physical geography, physics of the globe, and geophysics.

2. The earth astronomically and geodetically considered: (*a*) Origin and movements of the earth. (*b*) Density, gravity (experiments and results). (*c*) Movements of the earth's axis. (*d*) Origin of the tides. (*e*) Meteorites. (*f*) Experimental investigations.

3. History of principles and doctrines of geophysics (extracted from astronomy, meteorology, physics, physics of the globe, physical geography, and geology).

4. Helps and hints (auxiliary elements): (*a*) Geological and mineralogical chemistry. (*b*) General works on microscopic petrography. (*c*) Rock analysis. (*d*) Synthetical procedures (unity of forces in geology, unity of forces in nature, conservation and transformation of energy, unity of science).

5. Paleo-climatology: (*a*) General and synthetical. (*b*) Analytic.

6. Structural geology: (*a*) Sedimentation. (*b*) Metamorphism (mechanical, physical, chemical). (*c*) Epeirogeny. (*d*) Orogeny. (*e*) Isostasy. (*f*) Thermodynamics. (*g*) Experimental investigations.

7. Dynamic geology: (*a*) External forces. (*b*) Erosion. (*c*) Earth's crust. (*d*) Temperature changes in depth. (*e*) Interior of the earth. (*f*) Geological time.

8. Volcanology: (*a*) Theory of vulcanism. (*b*) Distribution of volcanoes. (*c*) General and synthetical works on volcanoes. (*d*) Particular and analytic works on volcanoes. (*e*) Theories of intrusion. (*f*) Geysers, hot springs, etc. (*g*) Experimental investigations.

9. Seismology: (*a*) Seismometry. (*b*) Earthquakes, generally and synthetically. (*c*) Earthquakes, particularly and analytically.

10. Glaciology: (*a*) Theories of glacial age. (*b*) Theories of glacial motion. (*c*) Experimental investigations.

11. Terrestrial magnetism.

12. Physical properties of minerals, rocks, and magmas: (*a*) Constants. (*b*) Fusion and solidification. (*c*) Rock synthesis. (*d*) Deformation. (*e*) Jointing and faulting. (*f*) Viscosity of magmas. (*g*) Diffusion of magmas. (*h*) Mineral solutions.

The number of titles entered on cards October 1, 1904, is 6,566. The first section of the bibliography, entitled "Synthetical Geophysics," is nearly ready, and will form a volume of about 200 pages. The bibliography will include references from the third century.

## SECONDARY GRANTS.

The following is a record of the grants, not already mentioned, made under the allotment of $200,000 for minor grants. A few reports on grants made in 1902–1903 are included, as the work under them was continued into the fiscal year 1903–1904:

### ANTHROPOLOGY.

**George A. Dorsey,** Field Columbian Museum, Chicago, Ill. Grant No. 97. *For ethnological investigation among the tribes of the Caddoan stock.* $2,500.

*Abstract of Report.*—As a result of the year's investigations the conditions of the investigation of the mythologies of the Caddoan tribes is as follows: The manuscript entitled "Traditions of the Skidi Pawnee" has been printed by Houghton, Mifflin & Company as volume 8 of the Memoirs of the American Folk-Lore Society; the manuscript entitled "The Traditions of the Arikara" has been printed; the manuscript containing the investigations among the Wichita, entitled "The Mythology of the Wichita," and embracing an extended introduction, which may be regarded as a preliminary report on the social organization of this tribe, has been submitted to the Institution, is being printed, and will soon be distributed; the investigation of the traditions of the Chaui, Kitkahahki, and Pittahauirata bands of the Pawnee has been completed and the manuscript will be prepared for the printer this winter; the investigation of the mythology of the Caddo is over half completed, will be continued during the early part of the coming year, and the manuscript will be submitted to the Institution some time next year.

The result of the investigations among the ceremonies of the tribes of the Caddoan stock is as follows: A preliminary but somewhat extended investigation has been made of the religious ceremonies of the Wichita; a large number of ceremonies not heretofore held for many years have been witnessed among the Pawnee proper; additional information has been gained about practically all of the great so-called "bundle ceremonies"; rituals filling about one hundred phonographic cylinders have been added, these covering some of the most important and most interesting ceremonies of the Skidi. Detailed information has been obtained of many of the most important Skidi ceremonies, especially the Medicine Men's ceremony and the ceremonies of the so-called "bundles" dedicated to the Morning and Evening Stars and to the institution of the office of warrior.

**William H. Holmes,** Director of Bureau of American Ethnology, Washington, D. C. Grant No. 44. *For obtaining evidence relative to the history of early man in America.* (Abstract of first report is in Year Book No. 2, p. xvi.) $2,000.

Mr. Holmes has not prepared a report for publication, but has placed the results of the preliminary survey in the hands of the Institution in such shape that it may be available in case the investigation is taken up later by the Carnegie Institution or by some other organization. He reports that no trace was found in any of the cave deposits of remains that can be safely attributed to a pre-Indian race or to a state of culture different from that of the known peoples of the region. The evidence as applied to the question of antiquity is therefore negative, but is nevertheless important, and will have value when we come to consider the history of the occupation of the American continent by primitive men. The collections made relate mainly to the American Indian, and a few fossil remains are included. The material collected has been deposited in the U. S. National Museum.

### ARCHEOLOGY.

**Frederick J. Bliss,** New York, N. Y. Grant No. 99. *For excavations in Syria and Palestine.* $1,500.

Dr. Bliss did not begin work in the field until September, so that his report is to the effect that he is in the field and ready to begin work.

**George F. Kunz,** New York, N. Y. Grant No. 52. *To investigate the precious stones and minerals used in ancient Babylonia, in connection with the investigation of Mr. William Hayes Ward.* $500.

*Abstract of Report.*—Mr. Kunz reports that his work thus far has been that of collecting literature and preparing himself to conduct the investigation when the work of Dr. William Hayes Ward is about completed.

**W. Max Muller,** Philadelphia, Pa. Grant No. 98. *For investigating monuments of Egypt and Nubia.* $1,500.

Before reaching Egypt Dr. Muller visited the museums at London and Oxford, England; Brussels, Belgium; Munich and Bonn, Germany; Vienna, Austria, and consulted with prominent Egyptologists. On arrival at Cairo he spent six weeks studying the contents of the great museum there. At Thebes two weeks were spent in making important observations, but severe illness, resulting from sunstroke, interfered greatly with the work which he expected to accomplish during the remainder of the season. From the material collected he expects to publish a volume which will be of much value.

**William Hayes Ward,** New York. Grant No. 131. *For a study of the oriental art recorded on seals, etc., from western Asia.* $1,500.

*Abstract of Report.*—During the summer of 1904 Dr. Ward was abroad, giving his entire time to work in the British Museum and other English and Scotch collections of seal cylinders. He secured a large number of casts and photographs. This supplemented the work he had done during the previous summer in Paris and Berlin.

Dr. Ward has now written nearly the whole of the analysis and description of the seal cylinders of the early and middle Babylonian empires, which covers more than half of the whole work. He has already prepared some 220 pages for printing. These include 26 chapters, with a full bibliography of the subject, an introduction on the origin, use, and materials of the cylinders, and a classification and explanation of the designs, with an identification of the gods figured and emblems employed. This begins with the most archaic period and carries on the development into the later conventional forms. Besides this text thus carefully prepared, he has selected from all published—and many unpublished—sources for these 26 chapters 375 cylinders, of which 320 have already been drawn. In addition a number of chapters have been written but not yet revised and copied for publication, and some 70 drawings have been made for other chapters. It is believed that the work will be completed in 1905.

## ASTRONOMY.

**Lewis Boss,** Dudley Observatory, Albany, N. Y. Grant No. 100. *For astronomical observations and computations.* (First report is in Year Book No. 2, p. xviii.) $5,000.

The program outlined in the preceding annual report has been followed, with some modifications of detail, throughout the year. Work has been prosecuted in two lines:

(1) In the section of observation, reductions of observations already made for the new Albany Catalogue have been carried nearly to the completion of the work. The observations for this catalogue were made at Albany in the years 1896 to 1901. This catalogue will contain about 10,000 stars, of which about 8,000 are in the zone —20° to —37° of declination. Every star in that zone denoted by reliable authority as brighter than magnitude 7.5 is included in the program, together with many other stars that are fainter. This catalogue can be made ready for publication very promptly at any time when means for its publication may become available.

(2) The main section of the work has been in continuation of that carried on in several previous years. This is the determination of star positions and motions from a homogeneous treatment of all material of observation readily available. This includes in the first line the standard stars which are the natural basis of the investigation. The results for 627 of the principal standard stars have already been published. A small volume containing these results, with an account of the investigation upon the systematic corrections in right ascension and declination for all the catalogues of observation, has been printed and will shortly be distributed.

During the year of this report the work on the standard stars has been extended. The positions and motions of a total of about 1,500 stars which may be reckoned in this class are now computed and are ready to be incorporated in a general catalogue. About two years ago the idea was entertained of forming a general catalogue of the brighter stars, together with other stars for which exceptionally accurate positions and motions could be computed.

Much work to this end had already been accomplished at that time. Later on this idea developed into the plan of including all stars down to the sixth magnitude, with the fainter stars already mentioned. Thus the work of preparation is going on for a general catalogue of all those stars. The positions and motions are computed with the same care as that which has been the rule for standard stars. Work on this line has been pushed with vigor during the past year. Special attention has been given to the revision of the systematic corrections employed as new material accumulates from time to time. The computations for a total of about 2,700 stars have been nearly completed, and work upon the remaining 2,300 is proceeding. It is hoped that the entire work will be ready for printing during 1905, and it is supposed that this general catalogue will include about 5,000 stars. Nothing of the kind has appeared since the publication in 1845 of the catalogue of the British Association. It is therefore believed that this catalogue will be found generally useful, apart from its primary design of furnishing a large number of systematically accurate observed motions of stars.

**W. W. Campbell**, Lick Observatory, Mount Hamilton, Cal. Grant No. 53. *For pay of assistants in researches at Lick Observatory.* (First report is in Year Book No. 2, p. xix.) $4,000.

*Abstract of Report.*—The expenditure of funds under this grant was made only as suitable assistants were procurable, and after living quarters on the mountain were constructed for their accom-

modation. One assistant, employed since May 8, 1903, in the meridian circle department, has been engaged in the reduction of the observations of the 2,800 stars in Sir David Gill's Zodiacal List. It is expected that the reductions will be completed in the summer of 1905. The purpose of the investigation is to supply more accurate positions of the principal stars near the paths of the planets of the solar system, to form a basis for improvements in their orbits. The assistant has also taken part, with Astronomer Tucker, in an extensive investigation of the division errors of the meridian circle by the method of simultaneous readings on both circles by the two observers, and he has made more than 9,000 circle readings for this purpose.

Three assistants have contributed, under the direction of Director Campbell, to the determination of stellar motions in the line of sight with the 36-inch equatorial and the Mills spectrograph; one assistant since July 17, 1903, and two assistants since June 20, 1904. The direct results of their work are as follows: The securing of 288 new spectrograms; the approximate measurement and reduction of 65 spectrograms; the definitive measurement and reduction of 240 spectrograms; the investigation of the micrometer screws of two measuring microscopes; the keeping up of the records of the investigation; the investigation of the temperature coefficient of the one-prism spectrograph, together with the design of a temperature case and thermostat; the investigation of the loss of light by absorption and reflection in the 36-inch objective and the correcting lens; the investigation of the loss of light by diffraction at the slit of the Mills spectrograph. An additional assistant in spectroscopy has been engaged for January 1, 1905.

**Herman S. Davis,** Gaithersburg, Md. Grant No. 102. *For a new reduction of Piazzi's star observations.* (First report is in Year Book No. 2, p. xix.) $1,500.

*Abstract of Report.*—Considerable work has been done toward determining the errors of adjustment of the meridian circle. Secular variations of precession in right ascension and declination have been computed for all stars by the method given in *A. N. 3965.* A critical discussion of the identity of all Flamsteed stars has been made for the column of star names in the final catalogue. Compilation has been made in form Z of all quantities thus far obtained (which will be published in the definitive catalogue), that they may be handy for reference as the work progresses. Explanatory introductions to many of the "forms" of manuscript have been written and the volumes bound.

**George E. Hale,** Yerkes Observatory, Williams Bay, Wis. Grant No. 103. *For measurements of stellar parallaxes, solar photographs, etc.* (First report is in Year Book No. 2, p. xx.) $4,000.

*Abstract of Report.*—This work has been carried on by Dr. Schlesinger, assisted by Miss Ware. The principal purpose of the investigation is to utilize the 40-inch telescope of the Yerkes Observatory for measuring the distances of a selected list of stars. The great focal length of the instrument and the possibility of obtaining well-defined stellar photographs with it particularly adapt it for this investigation.

The preliminary experiments demonstrated that the telescope could be used for photography without a color screen. They also showed that 8 by 10 inch plates would be required for the work. Accordingly, a special measuring machine, large enough to take plates of this size, was ordered from Gaertner and received in December, 1903. The various errors of the machine have been carefully investigated, and the instrument has proved to be well adapted to its purpose. Of the large number of photographs obtained during the year, 71 have been completely measured for the determination of parallaxes.

The preliminary reduction of the results indicates that they may be expected to yield very precise determinations of stellar parallaxes. In the case of the double star Struve P. M. 2164, the differences between the parallaxes of the two stars, amounting to 0.03, led to the discovery that the system is a true binary, in spite of the great separation and the faintness of the two stars forming it. The period of the system is probably between 350 and 400 years. Only two other binary systems are known that have greater separation of the companion stars, and both of these are much brighter than the pair under discussion. The corrected parallaxes for the system, as determined independently by the two observers, are in excellent agreement and have a very small probable error. This and other similar results are of special interest in showing the high degree of precision obtained in photographic measures made with a telescope constructed for visual observations only and employed in the present investigation without a color screen.

*Stellar Photometry.*—Mr. Parkhurst has continued his photometric observations with excellent results. In addition to the wedge photometry previously employed, he has had the use of a polarizing photometer kindly loaned by Prof. George C. Comstock, director of the Washburn Observatory. The 6-inch and 24-inch reflectors

and the 12-inch and 40-inch refracting telescopes have been used as heretofore.

The measurement of the magnitudes of the ninth and twelfth magnitude stars in the northern Rumford fields has been completed. There are still lacking nineteen sets to complete the work on the sixteenth magnitude stars with the 40-inch telescope. Work has been continued on a selected list of twenty-five variable stars, determining the light curves and the magnitude of the comparison stars. All the fields of the variable stars have been photographed with the 24-inch reflector, insuring a correct identification of the comparison stars and furnishing material for the determination of the photographic magnitudes. Other investigations include the photometric measurement of ninth to twelfth magnitude companions of some of the Struve double stars, made at the request of Professor Comstock, the measurement of standard stars in the Pleiades, the calibration of the wedge photometer by means of a polarizing photometer, etc.

In addition to several papers published in the Astrophysical Journal and the Astronomical Journal, Mr. Parkhurst has completed the manuscript of a large memoir, which includes a complete discussion of his investigations in stellar photometry and his observations of variable stars. This will be submitted to the Carnegie Institution for publication.

*Solar Investigations.*—The reduction of the Kenwood Observatory photographs of the sun, undertaken by Mr. Fox last year, has been completed by him. This yields the first determination of the rotation period of the sun as defined by the motion of the calcium flocculi. The new method of measurement employed, which involves the use of a globe upon which the photographs are projected, has proved to be very rapid and sufficiently precise for the purpose. The rotation periods of the calcium flocculi in different latitudes do not differ greatly from the results obtained by Stratonoff for the faculæ. The manuscript describing this investigation has been completed and will be submitted to the Carnegie Institution for publication.

Since completing these reductions in January, 1904, Mr. Fox has been in charge of the Rumford spectroheliograph, which is employed with the 40-inch refractor of the Yerkes Observatory. With this instrument he has obtained a large number of photographs of the calcium and hydrogen flocculi and of the prominences. He has also made photographs of the solar disk through certain dark lines of the solar spectrum, and has devoted special attention to a comparison of the photographs of the faculæ with photographs taken with the

second slit set on the $H_1$ band. The measuring globe has been readjusted for use with this series of photographs, and the preliminary reductions of the plates already measured shows that the rotation period will probably be in good agreement with that obtained from the Kenwood plates. On account of their larger scale, better contrast, and sharper definition, the photographs taken with the 40-inch telescope should yield results much more precise than those hitherto obtained.

Mr. Fox has also devoted some time to a photographic study of the spectrum of lightning and to the measurement of photographs of the spectrum of the spark, taken between iron poles in gases at high pressures.

**Simon Newcomb,** Washington, D. C. Grant No. 104. *For determining the elements of the moon's motion and testing law of gravity.* (First report is in Year Book No. 2, p. xxi.) $2,500.

The circumstance which gives importance to the research is the ascertained existence of inequalities of long period in the motion of the moon for which no explanation has yet been found. These inequalities are of such magnitude as to render impossible the prediction of precise positions of the moon for many years in advance, and their existence has been one of the two most perplexing problems of celestial mechanics during the last half-century.

To investigate the cause of these deviations, researches of two distinct classes are necessary. These are:

A. The computation from mathematical theory of the inequalities of long period which may be produced by the action of the planets. The problem involved in these computations is the most difficult and complex in celestial mechanics. Although it has been attacked by various authorities in recent times, it seems desirable, in view of the importance of the subject, to reconstruct the whole work by methods radically different from those hitherto adopted.

B. The comparison of the positions of the moon as computed from the tables, with astronomical observations of its position in the heavens. The observations best adapted to the present purpose are those of occultation of stars by the moon. In a work published by the Naval Observatory in 1878 Dr. Newcomb discussed all the observations of this class, as well as those of eclipses, from the time of the most ancient Babylonian records up to 1750. Much work was subsequently done in the Nautical Almanac Office toward continuing these computations to the present time. Dr. Newcomb's retirement from active service in the Navy having prevented the completion of

this work, an application was made to the Secretary of the Navy by the Carnegie Institution in December, 1902, for the use of the computations already made, in order that the work might be carried to completion under the auspices of the Institution. This request was complied with in March, 1903, and the work has since been prosecuted as rapidly as the limited time and means at Dr. Newcomb's disposal have permitted.

The work of class A is substantially completed for the action of all the planets which can affect the motion of the moon. The most that remains is to check some portions of the work by duplicate computations, and to compute the direct action of Saturn, which will probably prove too small to be of importance. It is interesting as showing the certainty obtainable in mathematical astronomy that the computation by the new methods, although radically different, almost from the first figure, from those previously made, have led to results substantially confirming those of Radau, whose investigations are the most complete heretofore made. The principal differences are that the more rigorous computation has shown a marked correction to the Jovian evection, due to the introduction of terms omitted by the other investigators. But nothing has been found which explains the observed inequalities of long period, and it is therefore probable that they can not be due to the action of the planets.

In the work of class B the computation of 567 occultations, made at various observing stations during the last seventy years, is nearly complete. That they are not completely finished is owing to a delay in procuring definitive positions of the occulted stars from the Nautical Almanac Office. This want has recently been supplied through the superintendent of the Naval Observatory, and the comparison will probably be carried to completion before the end of December. Besides these occultations, those observed at Greenwich and the Cape of Good Hope will be ultimately introduced. They have been already reduced and compared in the publications of the respective observatories. It is, however, necessary to transform the results of this comparison in order to adapt them to the present work. It is anticipated that before the end of the present calendar year the comparison of the tabular and observed places of the moon from the earliest Babylonian records up to the year 1903 or 1904 will be completed. What will then remain will be the introduction of a great number of small corrections to the tabular and observed positions and the discussion of the results with a view of determining the elements of the moon's motion. It is expected that this work will be completed during the year 1905.

Dr. Newcomb states that the execution of the work has been possible only through the great mathematical ability, expertness in astronomical computation, and general enthusiasm and fidelity of Dr. Frank E. Ross, recently appointed research assistant by the Carnegie Institution.

**W. M. Reed**, Princeton Observatory, Princeton, N. J. Grant No. 105. *For pay of two assistants to observe variable stars.* (For first report see Year Book No. 2, p. xxii.) $1,000.

*Abstract of Report.*—From March 1, 1903, to August 31, 1904, 17,112 settings have been made with the artificial star photometer attached to the 23-inch refractor of the Halsted Observatory. The observations have been made on 149 nights. The observing list has consisted, first, of those variable stars that have been reported monthly as faint by Prof. E. C. Pickering, director of the Harvard College Observatory; secondly, of certain stars selected as standards of magnitude that are now being observed by the Lick, Yerkes, and Harvard observatories; and, thirdly, of a few stars of special interest, such as Z Draconis and the companion of Polaris.

The present photometer was found inadequate to the study of the unique variations in Z Draconis that were discovered at this observatory last year. For that reason a new nickel prism photometer was ordered with a portion of the money granted by the Carnegie Institution for this year. The delay in securing the proper prisms from Germany was such that no observations have as yet been made with the new instrument. The reduction of the observations has been kept up to date and will be ready for publication as soon as a correct value for the scale of the photographically prepared "wedge" has been determined from observations upon the Pleiades.

**Henry N. Russell**, Cambridge, England. Grant No. 2. *For photographic determination of the parallaxes of stars.* $1,000.

The object of this work is to obtain by the photographic method determinations of the parallaxes of stars. The working list contains 76 stars, in 55 fields. Of these there are 29 stars of large proper motion; 21 whose parallaxes have been previously determined, but which are in need of revision; 17 binary stars, belonging to 12 systems; and 9 variable stars. Five stars, the parallaxes of which have been previously well determined, are selected as test objects and with a view of obtaining accurate positions for use in future investigations of secular variation of proper motion. Twenty-

eight of these objects are brighter than magnitude 4.5. These have been photographed with the aid of a color screen. The instrument employed is the Sheepshanks equatorial of Cambridge Observatory—a photo-visual Coudé refractor of 12 inches aperture and 20 feet focal length, at present set apart exclusively for this investigation.

The photographic plates are made especially for this observatory on plate glass. A *réseau* is employed. The color screen consists of a plate of plane-parallel glass, carrying a small rectangular patch of yellow collodion film, and is placed directly in front of the sensitive plate, so that the bright star is photographed through the film at a reduction of brightness amounting to six magnitudes. The plates obtained with this screen are highly satisfactory, the definition, if anything, being better than on ordinary plates, and there is no indication of sensible distortion.

The first plate for measurement was taken November 18, 1903. Up to June 28, 1904, 118 measurable plates, with four exposures on each, have been obtained of 47 fields, 45 of which are of bright stars. All photographs are taken within 30 minutes of the meridian.

At present 84 plates of 34 fields have been measured, and 64 plates of 24 fields have been completely reduced down to the formation of equations. The $x$-coördinate is alone to be discussed, thus halving the labor of measurement without sensible sacrifice of accuracy. Two of the four images of a plate are measured in the direct and the two others in reversed positions of the plate, thus halving the labor of measurement without material sacrifice of accuracy. Eight symmetrically disposed comparison stars on each plate have usually been measured. With few exceptions these are included between the eighth and tenth magnitudes. The use of a number of comparison stars facilitates recognition of a sensible parallax for any one of them. One such case has already appeared.

No attempt is made to deduce the standard coördinates of the plates from meridian observations, and the short methods of Turner and Dyson are employed in the reductions. These methods are justified on account of the care exercised to have the parallax star very near the center of gravity of position for the comparison stars.

The probable error of an $x$-coördinate derived from a single plate is about $\pm 0''.05$, deduced from comparison of pairs of plates.

In carrying on this work encouragement, criticism, and advice have been received from Mr. A. R. Hinks, the chief assistant of the Cambridge Observatory, as well as from others, to all of whom grateful thanks are due.

**Solar Observatory, Mount Wilson, Cal.,** Dr. George E. Hale, Director. Grants Nos. 70 and 185. $15,000.

As the result of the favorable report made by Professor Hussey in 1903, a careful test of the conditions for solar work on Mount Wilson (altitude, 5,886 feet) was undertaken by Dr. Hale in the winter of 1903–1904. In March, 1904, the work of erecting on the mountain a 15-inch cœlostat telescope of 61.5 feet focal length was undertaken. The instrument was ready for use early in April, and some excellent photographs of the sun were obtained with it. Since that time a Littrow spectroscope of 18 feet focal length has been employed with the telescope in a study of the spectrum of the flocculi; the resulting photographs are much superior to those previously obtained, and throw new light on the nature of the flocculi.

With the aid of meteorological instruments furnished by the Carnegie Institution, daily observations of the temperature, wind movement, and humidity were commenced in April and have been continued regularly ever since. These indicate remarkably favorable conditions for astronomical work because of the great amount of clear weather and the low humidity and wind movement. Up to September 1, 132 days out of 136 were suitable for observations. Daily observations of the sun have been made with a small telescope throughout this period. These show that the definition is superior to that of any other site with which Dr. Hale is acquainted. A complete report on these observations has been prepared and will be submitted to the Carnegie Institution.

In April, 1904, the Carnegie Institution made a grant to provide for the erection and use on Mount Wilson of the Snow cœlostat telescope of the Yerkes Observatory. Since that time the work of construction on the mountain has been pushed forward as rapidly as possible, and it is hoped that the instrument may be ready for use before the end of the present year. In order to transport the heavy parts of the instrument to the summit of the mountain it was necessary to widen and improve the narrow trail, over 9 miles in length, which leads to the valley. A special carriage was also constructed for this work, and at the present time this is making the round trip daily. Practically all of the heavy parts of the instrument, including the mirrors, are now at the summit of the mountain. The large stone piers required for the cœlostat and the solar spectroscopes have been completed, and the house which is to cover the instrument is being erected. A small machine shop, with gasoline engine and dynamo, has been constructed on the mountain to

use in conjunction with the telescope and to supply power for pumping water from the wells, which are 325 feet below the summit. A detailed report will be presented after the completion of the buildings and instruments.

**Mary W. Whitney,** Vassar College, Poughkeepsie, N. Y. Grant No. 23. *For measurement of astronomical photographs, etc.* (First report in Year Book No. 2, p. xxiii.) $1,000.

The work upon the catalogue of stars within 2 degrees of the North Pole, based upon photographs taken at Helsingfors, Finland, is nearly ready for publication. The preliminary catalogue was finished in the fall of 1903. The intercomparison of plates and the other processes leading to the final catalogue are completed. There remain some further consideration of magnitude and the final revision of manuscript and tables.

BIBLIOGRAPHY.

**Robert Fletcher,** Army Medical Museum, Washington, D. C. Grant No. 106. *For preparing and publishing the Index Medicus.* (First report is in Year Book No. 2, p. xxiii.) $10,000.

Since the last report, the volume of the Index Medicus for 1903 has been completed, and the annual index to the same has been issued. The latter consists of an index of authors in triple columns, and an index of subjects in double columns. In the second part, under appropriate headings, all the references in the year's volume are brought together for convenience of consultation. Of the present volume the monthly numbers from January to July, 1904, have been issued, and the number for August is nearly ready. It may be mentioned that as each number represents the literature of an entire month it can not be ready for delivery until the middle of the following month.

The scope of the work is very broad in its relation to medical science. It contains in classified form everything published throughout the world, month by month, which treats of medicine or public hygiene. The latter subject comprises all that concerns the public health in its municipal, national, and international relations. The work of biologic and pathologic laboratories, which are increasing in number in all the principal cities of the world and are of signal importance in the prevention of disease, forms a prominent part of the Index Medicus.

The subscribers to the journal are principally residents of the United States, but in the list are subscribers in Australia, Austria-

Hungary, Bohemia, Canada, Denmark, England, Finland, France, Germany, Ireland, Italy, Panama, Philippine Islands, Portugal, Roumania, Scotland, Spain, Sweden, Switzerland, and Wales.

**Ewald Flügel,** Stanford University, Cal. Grant No. 146. *For the preparation of a lexicon to the works of Chaucer.* $7,500.

*Abstract of Report.*—The work as planned is to be a lexicon to the works of Chaucer, based on the texts as published by the Chaucer Society. It aims to give scrupulously exact and complete quotations of all the words used in the genuine works of Chaucer, and in such of the so-called "spurious" works about which still "*sub judice lis est.*" It aims, further, to give full information as to the orthography and morphology of these words and their meaning, usage, and construction. The individual article will consist of a brief heading and a main part. The heading will consist of several paragraphs.

The first rubric is to be devoted to the orthography of the words. It is to give information about the different forms as they appear in the different manuscripts; about the dialectical and other peculiarities of scribes, etc.; about the rimes, if any; about the accentuation (it ought to give statistical information about changes of accent, as between natúre and náture, píte and pité, etc.).

The second rubric is to deal with the morphology of the words (*e. g.*, the parts of the verb, etc.).

The third with the etymology.

The fourth with the semasiology, with the meaning of the individual words in Chaucer's time whenever necessary. It will answer such questions as: "Is the word generally used in Chaucer's time, and in the same meaning in which Chaucer uses it? Is it an uncommon word or one with a special flavor? (Slang, courtier's word?) If it is a French word, what is the meaning of the word in contemporary Old French? How do Marchault, Deschamps, Froissart use the word? *e. g.*, What is the meaning of the French word 'armee' in Chaucer's time ('at many a noble armee had he be')? Is it a military expedition on land or sea? Is 'arrive' (the reading of some MSS.) a French word in Chaucer's time? What does 'presse' mean in Old French ('Flee from the presse')? etc. Does 'gouernance' mean 'self-control' (as Skeat has it), or 'conduite' (as the French usage of Chaucer's time proves it)? Does 'Regalye' mean 'rule, authority' or rather 'royal prerogative,' 'royal dignity,' etc.?"

The second main part of each article is to be devoted to *Chaucer's* use of the word.

(*a*) Here the quotations are to be arranged chronologically (as far as possible), beginning with the earlier works and ending with the later ones.

(*b*) The whole material of the quotations to be arranged historically, and not primarily critically. Words, *e. g.*, of Latin origin, like "honour," "religioun," "honeste," are to be arranged so that *not* the original, *classical* meaning of the Latin word is to lead, but that meaning which the word had in contemporary French (from which Chaucer took it). In other cases this may be different.

(*c*) Special attention in quoting is to be given to the construction of the words, phrases, etc. As an example, the author will quote "suffyse to thy thyng," "suffise to thi god though it be small," and "suffise the thyn owne"—the first construction to be found in Gower and Occleve, but a Latinism, and a Late Latinism at that.

The proper names will be in the main alphabet, but the author is undecided about the admission of the MS. "headings" and MS. "colophons" of the poems, etc. He is inclined either to give them in smaller type or in a special alphabet at the end of the book. The Latin and French quotations, the marginal glosses of some MSS. of the Canterbury Tales, etc., are to be given in an appendix.

In order to achieve all this, the collections should contain :

First, complete references to all the words of Chaucer's works, their various forms and all the accessible variants. The "spurious" works, as far as there are still dissenting views among the scholars as to their authenticity—as far as there is still a shadow of doubt as to the possibility of their being Chaucer's—are to be treated as carefully as the "genuine" works; but typographically these quotations are to be differentiated, making a comparison with the genuine words easy typographically for the eye, and instructive.

Secondly, these collections should contain a sufficient collateral apparatus of quotations from Chaucer's contemporaries and immediate predecessors, in Middle English and Old French ; in some cases of Late Latin authors.

**Herbert Putnam**, Washington, D. C. Grant No. 107. *For preparing and publishing a handbook of learned societies.* (First report is in Year Book No. 2, p. xxiv.) $5,000.

The compilation of the handbook has been under the immediate direction of Mr. J. D. Thompson, in charge of the Science section,

Library of Congress. The work done during the past year has consisted chiefly in—

(1) Endeavors to secure information about societies and institutions which did not reply to the circular letters sent out in 1903, viz:

(*a*) Personal investigations in Europe: (1) By Mr. and Mrs. J. D. Thompson (Great Britain, France, Belgium, Holland, Germany, Italy, Switzerland). (2) By Mr. A. V. Babine (Russia, Austria-Hungary). (3) By Mr. A. R. Spofford (Spain, Italy). (4) By Mr. J. Dieserud (Norway, Sweden, Denmark).

(*b*) Assistance by the United States diplomatic service in South America.

(*c*) Further efforts by correspondence.

(2) Reducing to standard form the material received, at the same time verifying the statements made and supplementing them, whenever inadequate, by reference to bibliographies and other publications in the Library of Congress.

It is expected that the first part of the handbook will be ready for printing in November, and that the manuscript of the remaining parts will be completed within the two years allotted for compilation. Arrangement of the societies by countries, with a subject index, is proposed, in place of classification by subject, as originally approved.

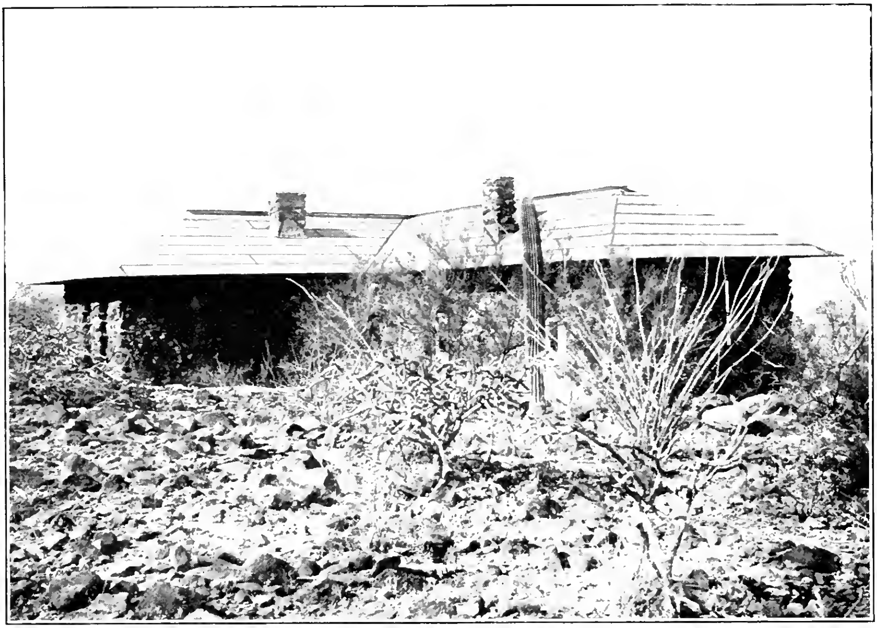
## BOTANY.

**Desert Botanical Laboratory.** Grant No. 108. Frederick V. Coville, Washington, D. C., and D. T. MacDougal, New York, N. Y., Advisory Committee. $5,000.

*Abstract of Report.*—Dr. MacDougal, of the Advisory Committee, was occupied at the laboratory during the month of February, 1904, in planning and carrying forward an investigation of soil temperatures. Continuous observations have since been carried on by means of the instruments installed at that time. In addition, Dr. MacDougal made an examination of the vegetation of the delta of the Colorado River and of adjoining portions of Lower California. His papers on the latter work are enumerated below in the list of publications emanating from the laboratory.

Dr. Coville, of the committee, visited the laboratory in June and conferred with the resident investigator regarding the work and the business affairs of the establishment.

Dr. W. A. Cannon, the resident investigator, has developed methods and apparatus for the quantitative measurement of transpiration in plants *in situ*. He has prepared a paper describing the apparatus



DESERT BOTANICAL LABORATORY. A REAR VIEW, LOOKING NORTHWESTWARD.

DESERT BOTANICAL LABORATORY. VIEW IN REAR OF THE BUILDING, LOOKING SOUTHWESTWARD.

and showing the application of the methods devised, which will later be offered for publication. Incidentally, he has prepared and published a paper on the germination of the desert mistletoes, as given in the list on page 100.

Prof. V. M. Spalding, of the University of Michigan, was occupied at the laboratory from October, 1903, until April, 1904, in an investigation of the biological relations of the creosote bush and

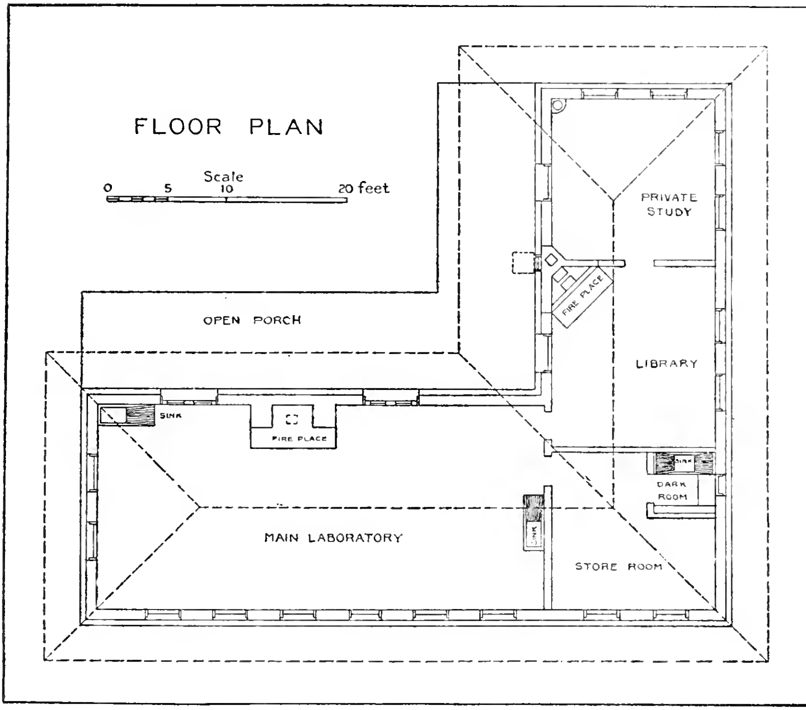



FIG. 5.—Floor plan of Desert Botanical Laboratory.

other desert shrubs. By means of the apparatus developed by Dr. Cannon, Professor Spalding ascertained that the creosote bush maintains a continuous transpiration in an adobe soil containing as low as 3 per cent. of moisture (air dried). This indicates an absorptive power far in excess of any heretofore recorded. (See page 100.)

Mrs. E. S. Spalding made observations on the giant cereus, and ascertained the manner in which it adjusts the diameter of its trunk to the varying amounts of water it is able to absorb and store. (See page 100.)

Dr. B. E. Livingston, of the University of Chicago, under a grant from the Carnegie Institution, spent July to September, 1904, at the laboratory, engaged in investigating various desert plants with reference to their power to abstract moisture from arid soils. The results of his work have not yet been formulated.

Prof. F. E. Lloyd, of Columbia University, was occupied at the laboratory from June to August, 1904, under a grant from the Botanical Society of America, in studying the comparative anatomy of desert plants and the relation of their stomatal action to transpiration. The results are to be incorporated in a paper now in preparation.

Following is a list of titles of papers descriptive of investigations carried on in connection with the Desert Botanical Laboratory during the past year :

CANNON, W. A. Observations on the germination of *Phoradendron villosum* and *P. californicum*. Bull. Torr. Bot. Club, 31 : 435–443. 6 figs. 1904.

MACDOUGAL, D. T. Botanical explorations in the southwest. Jour. N. Y. Bot. Gard., 5 : 89-91. 1 pl., 5 figs. 1904.

MACDOUGAL, D. T. Delta and desert vegetation. Bot. Gaz., 38 : 44–63. 7 figs. 1904.

SPALDING, V. M. Biological relations of certain desert shrubs. The creosote bush (*Covillea tridentata*) and its relation to the water supply. Bot. Gaz., 38 : 122–138. 7 figs. 1904.

SPALDING, EFFIE SOUTHWORTH. Mechanical adjustment of the saguaro (*Cereus giganteus*) to varying quantities of water. To be printed in the Bulletin of the Torrey Botanical Club.

Several applications for the privileges of the laboratory during the coming year have been received.

A small storage building has been erected near the laboratory. Electric fittings have been put into place, and necessary additions have been made to the apparatus and equipment.

**Burton E. Livingston,** University of Chicago, Chicago, Ill. Grant No. 156. *For investigations of the relations of desert plants to soil moisture and to evaporation.* $400.

Dr. Livingston's investigations were carried on at the Carnegie Institution Desert Botanical Laboratory at Tucson, Arizona. The work has been carried on by quantitative measurements of several phenomena, the data of which have not yet been brought to a condition to warrant more than a general statement. The months of July, August, and part of September were spent at the Desert Botanical Laboratory in making these measurements. The latter part of September was spent in a town in the dry region lying still farther west, and studies were made at several points in California.

DESERT BOTANICAL LABORATORY, TUCSON, ARIZONA. A FRONT VIEW. THE BUILDING FACES NORTH.

The work may be outlined as follows:

(1) The amount of water in desert soils was determined by samples both after a long period of drought and after rains. The amount of water at the dryest season, at no great depth, is surprisingly great. After four to five weeks without rain the soil in the openings between rock fragments, at a depth of 40 centimeters, was found to contain water to the extent of 10 to 12 per cent by volume. These observations were made on the shoulder of Tucson Mountain, near the Desert Laboratory.

(2) The retaining power of adobe clay for water was measured and found to be 50 per cent by volume.

(3) A piece of apparatus was devised to measure the natural evaporation by short periods, and a curve was constructed for several weeks. This rate was related to the loss by a free water surface, soil of various degrees of moisture, sugar solutions, the leaves of several desert plants, etc.

(4) The sensible temperature was recorded by short periods for several weeks. The importance of wind in lowering this and in raising the rate of evaporation is emphasized by the results.

(5) The amount of water necessary to promote germination in several seeds was determined, as was also the degree of dryness that could be withstood by several desert plants.

(6) The concentration of the juices of several desert plants was found to be little or no higher than that of ordinary plants. The amount of mucilage in the sap may have to do with retaining the water. Further experiments on the relation of mucilage to evaporation of its solution will be carried out.

(7) The resistance of soils of varying degrees of moisture to absorption by roots was determined by several methods, and this reduced to terms of osmotic pressure.

(8) The power of a soil to absorb water from a moist atmosphere was measured in several cases.

(9) The rate of transpiration of small plants (per unit leaf surface) was determined during periods of several days, ending in the wilting of the plant from lack of water.

**E. W. Olive**, University of Wisconsin, Madison. Grant No. 132. *For researches on the cytology of certain lower plants.* (First report is in Year Book No. 2, p. xxvii.) $1,000.

*Abstract of Report.*—Six distinct lines of research are in progress, with a view of determining, if possible, the origin in the lower plants of the complicated cell conditions found in the higher organisms.

The subjects include: (1) The cytology and development of Diplophrys. (2) The morphology and development of Ceratiomyxa. (3) On the cell organization of the larger bacteria. (4) On the cytology of various blue-green algæ. (5) On the cytology and general morphology of various species of the Entomophthoreæ. (6) The morphology of *Monascus purpureus*.

One paper on the blue-green algæ is in press, another on Monascus is almost ready for the publishers, while considerable progress has been made, particularly on problems 2 and 5. Of special interest is the discovery that the nuclei of the blue-green algæ are, under ordinary conditions, in a state of continuous mitotic activity, the division occurring with more or less rhythmic regularity. Further, the large nuclei of Empusa appear to present a somewhat new type of karyokinetic division. They possess intranuclear division centers and their minute chromatin granules do not become aggregated into definite chromosomes.

**V. M. Spalding,** Tucson, Arizona. Grant No. 189. *For investigation of absorption and transpiration of water by desert shrubs.* $600.

*Abstract of Report.*—(1) The creosote bush maintains life for long periods in a soil which gives up on drying no more than 3 per cent of water; it also grows in completely saturated soil. Plants grown in pots three months, which were supplied with 53 ounces of water during that period, made a scarcely less vigorous growth than one which received 110 ounces in the same time. An accumulation of similar facts will make it possible to give quantitative expression to the power of adaptation of this species to extreme conditions of water supply.

(2) The creosote bush maintains regular transpiration after long periods of excessive drought. Experiments during the present year have shown that the rate of transpiration is determined primarily by the amount of water available in the soil. The action of other factors is conditioned upon this. Thus direct sunlight accelerates the rate of transpiration if the plant has a full supply of water, but not otherwise.

(3) As indicated by plasmolysis, the actively absorbing cells of the roots are capable of taking up water with a force equivalent to upward of ten atmospheres.

(4) The production of root-hairs is increased within certain limits by lessening the water supply. Regeneration of root-hairs takes place when water is abundantly supplied to a plant that has been living in dry soil.

CHEMISTRY.

**John J. Abel**, Johns Hopkins University, Baltimore, Md. Grant No. 109 (continuation of grant No. 24). *For study of the chemical composition of the secretion of the supra-renal gland.* $500.

*Abstract of Report.*—Assisted by Mr. R. de M. Taveau, Dr. Abel has continued his investigations on the chemical constitution of epinephrin and of epinephrin hydrate (adrenalin, suprarennin). Carefully conducted oxidation of both epinephrin and its hydrate with dilute nitric acid led to the formation of large amounts of oxalic acid; also of a peculiar and hitherto unknown basic substance having the composition represented in the formula $C_3H_4N_2O$. On treating this base with fixed alkalies it is decomposed and yields ammonia, methylamine, and methylhydrazine. The occurrence of methylhydrazine among these products leads the writers to conclude that the two nitrogen atoms of the new base, $C_3H_4N_2O$, are directly linked to each other. More work, however, needs to be done before this deduction can be made to apply to the nitrogen of epinephrin itself.

The action of fused alkalies on epinephrin hydrate has also been studied. Skatol, which the writers had heretofore observed among the fusion products of monobenzoyl epinephrin, was now easily obtainable. A substance having some of the properties of protocatechaic acid and yet differing from this acid in certain respects was also obtained on fusion with sodium amalgam. This aromatic derivative is still under investigation.

An adequate constitutional formula for epinephrin must explain not only all of the ordinary reactions of this substance, but also the formation of the degradation products just enumerated. The formulæ that have been recently proposed fail to meet these demands, being, for example, unable to account for the appearance of the base, $C_3H_4N_2O$, among the oxidation products of epinephrin. The writers entertain the hope that further experimentation will enable them to offer a formula which shall more correctly represent the constitution of epinephrin.

As the correctness of the empirical formula, $C_{10}H_{13}NO_3 \frac{1}{2} H_2O$, for epinephrin hydrate has recently been challenged by European investigators, the writers are now engaged in a redetermination of this formula. In order to obviate possible errors due to oxidation from contact with the air, the whole process of isolation and all the steps of purification are being carried on in an atmosphere of hydrogen. This work is well on the way to completion, and the results will soon be published.

**Wilder D. Bancroft,** Cornell University, Ithaca, N. Y. Grant No. 140. *For a systematic chemical study of alloys.* (First report is in Year Book No. 2, p. xxix.) $500.

*Abstract of Report.*—During the year the equilibrium relations for the copper-zinc alloys have been determined. The two metals form no compounds. The freezing-point curve has six branches, each one corresponding to a series of solid solutions. Following the example of Heycock & Neville, these have been called the $\alpha$, $\beta$, $\gamma$, $\delta$, $\varepsilon$, and $\eta$ crystals, beginning at the copper end. Below about 450° the phase $\delta$ is instable, and only five series of solid solutions occur. The $\alpha$ crystals change with increasing content of zinc from the red of copper to a full yellow. The $\beta$ crystals are distinctly redder than the $\alpha$ crystals with which they can coexist. The other solid solutions are silvery in color. Since the $\beta$ crystals are ductile and the $\gamma$ crystals are very brittle, a brass containing 41 per cent of copper has a silvery fracture, while the polished surface is a pale yellowish red. The ingot breaks along the $\gamma$ crystals, while polishing emphasizes the $\beta$ crystals.

The conclusions from the temperature measurements have been confirmed by a careful microscopic study of the alloys. Forty-six photomicrographs are reproduced in the account of this work published in the June number of the Journal of Physical Chemistry.

Now that the equilibrium diagram has been finished, it will be possible to take up the study of the mechanical properties of brass and their variation with composition and heat treatment. The corresponding studies on the bronzes, reported under grant 176, have led to very interesting results. The work on the brasses will probably not yield such striking results, but it will be equally important as giving a rational explanation for the heat treatment.

In the report of last year there was submitted a provisional concentration-temperature diagram for the copper-tin-lead alloys. The work has been repeated so as to obtain more accurate freezing-point determinations. This has involved several changes in the recording pyrometer, and we now have an instrument which is inexpensive and yet capable of considerable accuracy. Automatic stirring and the addition of nuclei have been resorted to in all determinations. The more accurate results thus obtained have necessitated a revision of some portions of the diagram.

Another point has delayed the publication of this work. In the first report it was thought sufficient to accept Heycock & Neville's

incomplete conclusions as to the $\beta\ \gamma$ region of the equilibrium diagram. The mechanical tests which have been made this summer have shown that we must know the exact temperature-concentration limits for $\beta$, $\gamma$, and $Cu_4Sn$. Work on this is now under way, and it is expected that the report will be ready for publication before long. The experimental work has been done by Mr. E. S. Shepherd.

**Chas. Baskerville,** University of New York, New York City. Grant No. 113. *For investigations of the rare earths.* $1,000.

*Abstract of Report.*—The complexity of thorium has been demonstrated. This may be shown by several methods, among which are fractional precipitation with phenylhydrazine and fractional distillation of the chlorides in the making direct from thorium oxide. A very volatile portion distils over during the passage of dry chlorine over a mixture of the pure oxide and carbon; it may be collected in part by cooling and completely by absorption in alcohol. Thorium chloride at this temperature (760° C.) is sublimed within the apparatus, while a residue remains in the carbon-boat which contained the original mixture. This residue may be converted into an oxide, which is soluble in concentrated hydrochloric acid. Neither the original thorium preparation, nor the newer, purer compound, nor the volatile portion is soluble in this reagent.

The oxides from these three substances vary in their appearance, specific gravities, and atomic mass values as determined. Further differences—as, for example, radio-activity—were also noted, and are stated in a communication published in the Journal of the American Chemical Society. A number of organic and other salts of the new elements (carolinium and berzelium) have been prepared. We wish next to investigate these and obtain the elements in metallic form.

Using the apparatus purchased by the grant, we have been investigating the nature of neodidymium and præseodymium, the complex nature of which has been predicted by several workers. So far success has not attended this.

**Gregory T. Baxter,** Cambridge, Mass. Grant No. 154. *For research upon the atomic weight of manganese.* $500.

This work is to be carried on by a laboratory assistant during the college year 1904–1905. Therefore there has hardly been an opportunity to begin it. Some preliminary work has been done, but a definite report can not be made at this time.

**Moses Gomberg** and **Lee H. Cone**, Ann Arbor, Mich. Grants Nos. 78 and 153. *For study of triphenylmethyl and analogous compounds.* $500.

*Abstract of Report.*—Work under this grant was begun in October, 1903. A study of the physical properties of triphenylmethyl was first taken up. Since the compound is very readily attacked by the oxygen of the air, several pieces of special apparatus had to be devised for carrying on this work. By their use it was possible to determine upon pure samples the following constants of triphenylmethyl: The solubilities, the melting point, the molecular weight in several different solvents, and the electrical conductivity when dissolved in liquid sulphur dioxide. The results obtained were published in the Berichte d. deut. chem. Ges., vol. 37, pp. 2033–2051.

As an introduction to the study of the derivatives of triphenylmethyl with oxygen compounds, such as ethers, aldehydes, etc., the effect of oxygen itself upon the hydrocarbon was first fully investigated. The behavior of the peroxide so formed toward a number of different reagents was also worked out. The results of this work, together with a short preliminary notice as to the effect of sunlight upon triphenylmethyl and its analogues, is now ready for publication.

The determination of the energy relations between hexaphenylethane and triphenylmethyl is of special interest. M. Jules Schmidlin, in the laboratory of M. Berthelot, has kindly offered to make the requisite thermochemical measurements. Pure samples of the compounds to be investigated have been prepared here and sent to him, and the work of making the measurements is now in progress. Other problems have arisen in connection with the work. They relate largely to the improvement of old and the development of new methods for the preparation of compounds of the type of triphenylchlormethane, such as halogen- and nitro-substituted derivatives. This part of the work has not yet been completed.

**H. C. Jones**, Johns Hopkins University, Baltimore, Md. Grant No. 180. *For investigations in physical chemistry.* (First report is in Year Book No. 2, p. xxx.) $1,000.

*Abstract of Report.*—The investigation was carried out with the assistance of Dr. F. H. Getman, Carnegie Research Assistant.

During the past year a study of about eighty electrolytes and a dozen non-electrolytes with respect to their power to lower the freezing-point of water has been made. A dozen or more solutions of every one of these substances, varying in concentration from two or

three times normal to a few hundredths normal, have been employed and the molecular lowering of the freezing-point of water produced by them has been determined. The refractivities, densities, and conductivities of the above solutions have also been measured. In all, more than a thousand solutions have been brought within the range of this investigation. The results all point to the correctness of the theory advanced some three years ago by Dr. Jones, that in concentrated solutions of electrolytes there is combination between the dissolved substance and the solvent. *There are hydrates formed.*

A general relation was established between the amount of water of crystallization of electrolytes and the magnitude of the freezing-point lowering produced by them. The two were shown to be approximately proportional to one another. This is a necessary consequence of the theory of hydration in concentrated solutions and a beautiful confirmation of it. Those substances that crystallize with the largest amounts of water of crystallization would be the substances that in solution would hold the largest amounts of water in combination, and this would manifest itself by abnormally great freezing-point depression; and such is the fact. An enormous field of work is thus opened up, which will be pushed forward as rapidly as possible.

**W. L. Miller**, University of Toronto, Toronto, Canada. Grant No. 155. *For the study of electric migrations in solutions of weak acids.* $500.

Professor Miller submitted an abstract of a long report by Mr. W. J. McBain, who conducted the experiments on the electric migrations in solutions of weak acids.

*Abstract of Report.*—Mr. McBain has determined the transport in half-, tenth-, and fiftieth-normal acetic acid, and in tenth-normal propionic acid, and finds about 0.3 as the transport number for the acet-ion and the propion-ion in place of 0.1, as called for by the Theory of Solutions. Experiments with solutions in which acetic acid was mixed with sodium acetate or sulphuric acid show that the "undissociated" acetic acid does not move during the electrolyses; and this conclusion is confirmed by experiments with solutions of cadmium sulphate in mixtures of acetone and water, where the acetone was found to remain practically stationary during the electrolyses. An attempt to reconcile these results with the theory by assuming "hydrated" ions led to the conclusion that the hydrogen ion must be hydrated (at least 90 $H_2O$ for each H) in decinormal acid, and

that its degree of hydration must depend on the dilution of the acid—a conclusion which deprives the hypothesis of all value.

As regards the trustworthiness of the results, Mr. McBain is a very careful and able workman, and it is obvious from the report itself what a great deal of time and trouble he has devoted to these measurements. He himself is quite convinced of the reliability of his results, and if they were not in direct conflict with a generally accepted theory I imagine no one would call them in question. However, as it seems incautious to base wide-reaching generalizations on experiments in which so much depends on the manipulation, I have arranged with Mr. Dawson to make a study of the decomposition of acetic acid at the cathode, in the hope that it may prove possible to dispense with "protecting solutions" at that electrode, in which case the apparatus and manipulation would be much simplified and Mr. McBain's measurements could be checked by new experiments. Mr. Dawson will also experiment with various soluble anodes with the same object in view.

**H. N. Morse,** Johns Hopkins University, Baltimore, Md. Grant No. 110. *For development of a method for the measurement of osmotic pressure.* (First report is in Year Book No. 2, p. xxx.) $1,500.

*Abstract of Report.*—The work of Professor Morse and Dr. J. C. W. Frazer, his assistant, during the past year has been along two quite distinct lines. It had been found that a porous wall, which affords an effective support for the osmotic membrane, is sometimes produced at the potteries, though rarely, and then in only a few out of many cells, and it had been discovered, through a study of thin sections, that the structure of such porous walls differs in a characteristic manner from that of others which do not adequately support the membrane. The greater portion of the year has therefore been devoted to the molding and burning of different clays and clay mixtures and to a study of the properties of the products, the end in view being a discovery of the conditions which are favorable or unfavorable to the production of that peculiar structure of porous wall which is known to be essential. A large number of experiments of this kind have been made and a considerable amount of data accumulated which it is thought will be of use in the solution of the problem. The progress of the work has been considerably retarded by the necessity of devising and constructing new appliances, some of which involved a large amount of preliminary experimentation.

There has been developed simultaneously a system of laboratory heating by means of electricity which is believed to possess decided advantages over the methods in ordinary use. An account of the results obtained in this direction has been given, with the consent of the Carnegie Institution, in the American Chemical Journal, vol. xxxii, under the title "A New Electric Furnace and Various Other Electric Heating Appliances for Laboratory Use."

A. A. **Noyes**, Massachusetts Institute of Technology. Grant No. 45. *For researches upon: (1) Electrical conductivity of salts in aqueous solution at high temperatures; (2) Ionization of weak acids and bases and hydrolysis of their salts in aqueous solution at high temperatures; (3) Transference determinations in aqueous solutions of acids.* $1,000.

*Abstract of Report.*—These three researches have been carried out during the past year in the Research Laboratory of Physical Chemistry of the Massachusetts Institute of Technology. The first was executed with the assistance of Mr. Arthur C. Melcher; the second with that of Dr. Hermon C. Cooper; and the third with that of Mr. Yogoro Kato. The work upon all these investigations has progressed so far that three articles describing the methods and results will soon be submitted to the Carnegie Institution for publication.

The first investigation on the electrical conductivity of aqueous solutions at high temperatures has consisted thus far in the measurement of the conductivity of six salts—sodium chloride, potassium chloride, silver nitrate, barium nitrate, potassium sulphate, and magnesium sulphate—at the four temperatures of 18°, 100°, 156°, and 218°, and at the four concentrations of $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{80}$, and $\frac{1}{500}$ normal. The apparatus and the method employed were nearly identical with those described by Noyes & Coolidge.* The measurements with potassium and sodium chloride were to some extent a repetition of those of these investigators; they were made in order to estimate the probable accuracy of the results, which could well be done, since the new determinations were made by another experimenter in an entirely new apparatus of a different resistance-capacity. The new results even at 218° agreed with the old ones within 0.2 per cent. The results obtained with all these salts justify the conclusions: (1) That the degree of dissociation always decreases greatly with rise of temperature; (2) that this decrease is much larger for salts of the uni-bivalent type than for those of the uni-univalent type,

* Proc. Am. Acad. Arts and Sciences, *39*, 163 (1903).

and still larger for those of the bi-bivalent type; (3) that different salts of the same type have roughly the same degree of dissociation at high temperatures, just as they do at ordinary temperatures; and (4) that the migration-velocities of different ions approach equality with rising temperature. The conductivity of magnesium sulphate passes through a maximum between 130° and 155°, showing that in this case the decrease in dissociation is great enough to compensate the increase in migration velocity.

The second research, upon the hydrolysis of salts at high temperatures, has thus far been confined to one salt, sodium acetate, at temperatures extending up to 218°. The determination of the hydrolysis of this salt involved, however, not only measurements of its own conductivity at various concentrations, but also those of solutions of acetic acid, hydrochloric acid, and sodium hydroxide. The method in principle consists in determining the decrease in conductivity of sodium acetate produced by the addition of acetic acid to its solution. This decrease arises from the driving back of the hydrolysis of the salt by the excess of acid and the replacement of sodium hydroxide by a corresponding quantity of the more poorly conducting sodium acetate. The final calculations have not yet been made; but the results show that this salt, which at 25° in $\frac{1}{100}$ normal solution is hydrolyzed to an extent of only about 0.03 per cent., at 218° has a degree of hydrolysis of about 0.5 per cent. From the data the degree of dissociation of water itself will be calculated; these results already show that it is many times greater at 218° than at 18°. At high temperatures the phenomenon of hydrolysis therefore plays a most important part in determining the condition of salts in solution. The conductivity measurements incidentally made with hydrochloric and acetic acids have also an interest of their own; they show that the dissociation of both these acids, like that of the neutral salts, decreases markedly with rising temperature.

The third research consisted of about forty electrical transference experiments at 20° with very dilute hydrochloric and nitric acids. The object of them was to determine the electrical conductivity of the hydrogen ion—a constant of fundamental importance in applications of the ionic theory, since it is involved in the calculation of the degree of dissociation of all acids. The transference numbers obtained with $\frac{1}{500}$ normal hydrochloric acid are nearly identical with those previously obtained with $\frac{1}{180}$ and $\frac{1}{160}$ normal acid by Noyes & Sammet.* They therefore confirm the conclusion pre-

---

*J. Am. Chem. Soc., *24*, 944 (1902); *25*, 165 (1903).

viously drawn that the conductivity of the hydrogen ion derived from transference experiments is about 4 per cent. higher than that obtained by conductivity measurements. This conductivity value for the hydrogen ion was computed simply by multiplying the conductivity of the chlorine ion, as determined by Kohlrausch from the data for neutral salts, by the ratio of the transference numbers for the cathion and anion of the acid. The experiments with nitric acid were made in order to see whether independent transference determinations with a different acid would lead to the same conductivity value for the hydrogen ion. This was found to be the case: the transference numbers obtained with $\frac{1}{180}$ and $\frac{1}{500}$ nitric acid give a conductivity value corresponding within 1 per cent to that derived from the transference experiments with hydrochloric acid at the same concentrations. The discrepancy between the result obtained by this method and that by the conductivity method remains to be explained.

**Thomas B. Osborn**, New Haven, Conn. Grant No. 192. *For research on chemical substances yielded by proteids of the wheat kernel when decomposed by acids.* $1,500.

*Abstract of Report.*—The object of this investigation is to determine the nature and proportion of the different amine acids yielded by hydrolyzing the several protein bodies contained in the wheat kernel. As this investigation was but recently begun, the work has at present extended only to the preparation of a considerable quantity of pure gliadin and glutenin and the determination of the amount of glutaminic acid which several fractions of the former have yielded when decomposed by boiling acid. As the individuality of gliadin has recently been called in question by Kutscher, on the ground of different yields of glutaminic acid which he obtained, especial attention was necessarily first directed to this point. Dr. Osborn found that the actual yield of glutaminic acid is far in excess of that obtained by Kutscher from any of his fractions, and that the differences which he observed were due to faulty determinations of the amount of this substance.

Although extensive fractionations were carried out, no evidence of more than one protein substance, soluble in alcohol, was obtained, so that this investigation, together with the work done in past years in Dr. Osborn's laboratory, shows gliadin to be one of the best characterized and most definitely established protein substances accessible for investigation on a large scale. His present work has also shown that gliadin yields a larger proportion of glutaminic acid than any

other protein heretofore examined, namely, over 39 per cent. This large proportion of glutaminic acid is a matter of great importance in relation to the nutritive value of a food protein of such extensive use as gliadin, which forms about one-half the protein substance of wheat flour. The amount of glutaminic acid obtained from gliadin exceeds that of any single decomposition product as yet isolated, in a condition of established purity, from any other true protein body, and it is consequently a matter of interest in connection with the chemistry of the protein substances.

**Theodore W. Richards,** Harvard University, Cambridge, Mass. Grant No. 112. *For investigation of the value of atomic weights, etc.* (First report is in Year Book No. 2, p. xxxii.) $2,500.

*Abstract of Report.*—The researches conducted under the direction of Professor Richards during the years 1904–1905 were four in number, as follows:

(1) An investigation of the atomic weight of sodium, carried on with the assistance of Roger Clark Wells. Many unusually precise analyses led to the detection of small errors in the methods of Stas. The new values found were 23.015 and 35.467 for sodium and chlorine respectively. The first stage of this work is nearly ready for publication.

(2) A continuation of the study of the compressibility of elements and simple compounds, carried on with the assistance of Frederic Bonnet, jr. The elements studied were lithium, sodium, potassium, aluminum, and iron. The method of Richards & Stull, already published by the Carnegie Institution, was used in these determinations, with slight modifications demanded by the nature of the materials.

(3) An investigation of the effect of pressure and strain on the electromotive force of pure iron immersed in solutions of its salts, carried on with the assistance of Gustave E. Behr, jr. This investigation has already led to interesting results, but the experimental work is not yet finished.

(4) A research upon the electromotive force of cells composed of amalgams of different strengths, carried on with the assistance of George Shannon Forbes. The data found exceed in precision and comprehensiveness anything which has heretofore been attained in this direction. The first stage of the experimental work has been concluded and the details will soon be published.

All these researches will be continued during the coming year, with the assistance of the as yet unexpended balance of the grants.

**Henry S. Washington,** Locust, N. J. Grant No. 95. *For the chemical investigation of igneous rocks.* $1,200.

*Abstract of Report.*—The main objects of investigation were the leucite-bearing rocks of Italy, of which few satisfactory analyses exist. About twenty-five complete analyses were made by Dr. Washington in his own laboratory. The analyzed specimens were selected as representative of the various rock types which occur at each of the different centers of eruption, so that not only do the analyses express the range in composition of the different rocks, but the special features of each center, as well as the general characters of the Italian petrographical province. The rocks were found to fall into nine subranges, four of which are new. A special study was made of the types and habits, of which at least nine are well-defined and established. The constant presence of barium is a noteworthy feature, and is correlated with the high potash. Analyses were also made of some rocks from other localities of especial interest. The investigations, which are not quite complete, will be embodied in a monograph and one or two shorter papers, which it is hoped to complete by the end of the year. Only a small portion of the grant was used, as Dr. Washington was unable to go abroad to make special collections.

ENGINEERING.

**W. F. Durand,** Stanford University, California. Grant No. 64. *For experiments on ship resistance and propulsion.* (For first report, see Year Book No. 2, p. xxxii.) $4,120.

*Abstract of Report.*—The number of runs thus far made is 2,121, of which 1,216 belong to last season's work and 905 to the present season. These figures are, furthermore, exclusive of 228 runs made on a special model representing an annular element of a propeller blade. The work yet remaining will comprise the following items:

(1) About two hundred runs on regular propellers.

(2) About two hundred runs on a special model representing an element of an ideal propeller blade.

(3) The reduction of the observations made this season, and the final review of the entire series, with analysis of results in such ways as shall seem most useful for the purposes in view.

The accomplishment of these items will complete the investigation as originally laid out, covering the examination of 49 model propellers, and of two special models intended to elucidate certain points in connection with special phenomena encountered during the progress of the work.

**W. F. M. Goss**, Purdue University, Lafayette, Ind. Grant No. 114. *For a research to determine the value of high steam pressures in locomotive service.* $5,000.

*Abstract of Report.*—This work involves the operation of the heavy machinery making up the equipment of the locomotive laboratory of Purdue University, an organization of men supplementing the regular staff of the laboratory, the presence and assistance of university students, and an analytical study of experimental data. The outline provides for 76 formal tests of the university locomotive, 29 of which have now been made. Each test involves a run of approximately 100 miles.

Assistance has been given by the trustees and president of Purdue, by whose approval Professor Reynolds and his laboratory staff, while receiving but slight aid, have thus far carried on the tests; also by Mr. William Garstang, representing the Cleveland, Cincinnati, Chicago and St. Louis R. R. Company, who contributed four carloads of coal, amounting to 130 tons, in return for a report respecting its quality.

It is expected that the work of the laboratory will be completed by February next, and that the whole research will be finished and reported on by September, 1905.

### EXPERIMENTAL PHONETICS.

**E. W. Scripture**, Yale University, New Haven, Conn. Grant No. 121. *For researches in experimental phonetics.* (For first report see Year Book No. 2, p. xl.) $2,700.

*Abstract of Report.*—Among the results obtained, the following may be mentioned: The technique of speech recording and tracing has been developed to a high degree of accuracy. The method does for speech what microscopy does for tissues. Curves have been obtained of hundreds of American vowels for different speakers; also of various musical instruments.

Concerning the vowels, various hitherto unknown or uninvestigated properties were definitely established. The law of circumflexion in melody and of circumflexion in intensity for American vowels (previously discovered in my researches on Cock Robin record) was definitely established. The modification of this circumflexion for purposes of expression, the fusion of several circumflexions into a larger unit, etc., were investigated. The unified nature of a diphthong (as opposed to the view that a diphthong consists of two distinct elements) was established, as were also numerous

facts like the following: American long vowels need not be diphthongized; both short and long vowels may be diphthongized. The short vowels are often different from the ones supposed to be present. In the same word in similar phrases only a few seconds apart a speaker may use two utterly different short vowels. The number of typical vowels must be greatly increased beyond those recognized by the dictionaries. The short vowels are often incorrectly given in the dictionary pronunciations. A vowel is not a constant thing, but changes at every wave of its vibrations; it is an activity and not a dead product. The ear gets a general impression from the whole series of waves and can not distinguish the actual sound at any point. The ear is often misled in the rapidly changing short vowels. Sounds have no definite limits, but fuse more or less gradually into each other. The division of words into syllables and of verse into feet on present principles is nonsense, which can be avoided only by a new view (psychological and not typographical) of the nature of syllables and speech units. This new view (the centroid theory) is in accord with the experience of writers of verse (the attempt of modern writers on prosody to fit Greek and Latin notions to English verse results in a pedantic scheme of spelled verse that ignores the poet and the public, although it may please the printer); the frequent presence of "sonant h" in American English was proved. Various individual differences were investigated. Melody and intensity were found to vary in each vowel by different speakers.

The vowel curves showed that ordinary views of resonance could not be applied to speech; the vocal cavities have a soft wall. The laws of resonance for such cavities differ from those for cavities with hard walls. The glottal lips (which are masses of flesh, and do not in any way resemble cords or bands) emit series of puffs, and do not vibrate in the ordinary sense.

An investigation was conducted on the laws of resonators with soft walls (of water, gelatine, flesh, etc.) and on the action of puffs of varying sharpness. On the basis of the results a new vowel theory was elaborated. This theory finds its confirmation in the fact that all the vowels can be artificially produced by apparatus built according to it; and also in the fact that countless numbers of speech curves become for the first time intelligible when interpreted according to it. This theory takes account for the first time of the softness of the walls of the vocal cavities and of the flesh character of the glottal lips. It proposes the new notion that the muscular fibers in the *M. vocalis* of the glottal lips contract differently for each vowel, and

therefore alter the form of the puff (implying that the vowel involves not only special innervations to the mouth muscles, but also to those of the larynx). Interesting details concerning vowels of different speakers were found—*e. g.*, the strong chest tones of Jefferson and Depew, the strong mouth tone of Mitchell, etc.

Attempts were made to imitate the vowel curves by apparatus and by calculation on the assumption that if the results were good counterfeits the principles used in the apparatus or the computation must be valid for the vowels themselves. Principle after principle was tried. Good counterfeits were finally obtained. The principles found were used in developing the vowel theory just mentioned.

An apparatus was made to produce artificial curves on a gramophone disc. A speech curve of any kind could be taken and engraved on the disc. On placing the disc in the gramophone the sound is heard. This can be used to test any published curve. A zinc etching is made from it. This is used in the apparatus, and the sound is heard. This apparatus opens up an utterly new field, namely, that of the acoustic analysis of a vowel. Each wave for a vowel curve is engraved separately in repetition, and its sound is heard. Thus the sound of the vowel at each of its waves is produced. This apparatus produces a series of acoustic sections of the sound of a vowel, just as a microtome makes a series of sections of a tissue.

The studies of speech melody showed that the fundamental form for the American sentence is a circumflex melody, and that this is modified to suit each expression of thought or emotion (parenthesis, religion, ceremony, humor, etc.). Curves of the interjections illustrated how the melody was changed to express the emotion. Records of a German poem proved that it had a general type of melody—a specific melody—of its own, which showed itself in spite of the different melodies of different dialects of Germany.

These investigations form a unit and have to be carried along together. Every one of them is the first attempt to enter a new field either in any way or with accurate experimental records. Owing to the funds available and the concentration of effort, these investigations are now so far advanced that they can not be duplicated elsewhere for many years. The stimulus of the work is being felt in Germany. The melody investigations have been taken up in the University of Leipzig, and work on curves—obtained by Professor Scripture's apparatus—is being carried on in the University of Berlin. It is intended to make a unit of three years' work and publish the results in a volume of text with an atlas of plates containing speech curves.

## GEOLOGY.

**T. C. Chamberlin**, University of Chicago, Chicago, Ill. Grant No. 115. *For study of fundamental principles of geology.* $6,000.

*Abstract of Report.*—The main portion of the report consists of a statement of the work which has been done in the critical study and development of hypotheses relative to the earth's origin and its early stages. As the nature of the subject is such that the work done can only be definitely indicated by the results, an outline of these, of measurable fullness, is given. The work has been chiefly constructive, and has consisted mainly of (1) an unsuccessful attempt to develop a working hypothesis along the line of a meteoritic nebula of the quasi-gaseous type, (2) a definite development of a selected phase of the planetesimal hypothesis into working details, with applications to allied phenomena, and (3) a definite postulation of the stages of the earth's growth from its origin to the earliest known geological stage, the Archean, on the basis of the preceding hypothesis.

(1) The serious obstacles to the construction of a hypothesis having probable conditions and working qualities on the line of a meteoritic nebula of the quasi-gaseous type are set forth.

(2) In developing a definite phase of the planetesimal hypothesis, the effort has been to bring it into contact with related phenomena at so many points as to afford facilities for testing its verity, or, if not that, at least its temporary working qualities, by existing knowledge and the results of progressive investigation, and at the same time to stimulate, if possible, attention to the pertinent data and their significant bearings.

(3) The hypothetical stages of the earth's growth deducible from the special phase of the planetesimal hypothesis previously developed involve the origin of the atmosphere, of the hydrosphere, of the continental platforms and oceanic basins, of vulcanism, and of the larger phases of earth deformation. The last is not, however, sketched beyond its leading features, as further study is desired on an important feature recently developed and not as yet duly worked out.

The progress of the studies of collaborators is appended.

A communication from Dr. Moulton sets forth the state of his work in the critical discussion of the history of the nebular hypothesis and other theories of the origin of the earth.

A statement is also made by Dr. A. C. Lunn as to the progress of his inquiries into the generation of internal heat by the gravitational compression of the earth.

The state of the inquiry into the condition of the atmosphere at the time of the formation of certain gypsum deposits, conducted under the direction of Prof. Julius Stieglitz, is set forth in a communication from him.

The stage of progress of the study into the relations of tidal action to the rotation of the earth, in connection with Professor Slichter and other collaborators, is indicated.

**Bailey Willis,** U. S. Geological Survey, Washington, D. C. Grant No. 116. *For geological exploration in eastern China.* (For first report see Year Book No. 2, p. xxxv.) $12,000.

At the time of publication of the first report in Year Book No. 2 Mr. Willis had just gone into the province of Shan-tung. His present report covers the period of exploration in China and his return to the United States. The itinerary of the expedition is given in Mr. Willis's report, pages 275–291.

Mr. Willis submitted a summary of contributions to knowledge resulting from the expedition, comprising subjects in geology, geography, and zoölogy. Among the geological subjects there are three which are of chief interest: Cambrian faunas, glacial deposits of early Cambrian age, and the history of mountains in China as compared with that of mountains in America and Europe. The Cambrian fossils are most interesting, and give data for the correlation of the Cambrian faunas of America and China. The glacial deposit of Cambrian age is an almost unique discovery, equaled in interest in its way only by the extraordinary evidences of glaciation in southern Africa in Carboniferous time. The contribution to knowledge of mountains follows from an application of the principles of the modern science of physiography to new fields, and is one of the most far-reaching results of the expedition in its effect upon broad geological theories. Within a few years we have come to know that North America has, in the latest geological epochs, been the scene of vigorous mountain growth, probably not exceeded in activity in any past age. The studies of Davis in Europe and western Asia have indicated the existence of similar facts in those regions, and the latest European work is confirming his inferences. Mr. Willis has now extended the area of observations, with like conclusions, across Eurasia to the Pacific, and thus it is shown that in the northern hemisphere the features of the earth's surface express recent activity of vigorous internal terrestrial energy. The effect of such a conclusion upon the theories of a nearly exhausted earth is important.

GEOPHYSICS.

**Frank D. Adams**, McGill University, Montreal, Canada. Grant No. 117. *For investigation on flow of rocks.* (For first report see Year Book No. 2, p. xxxiv.) $1,500.

*Abstract of Report.*—The experimental work carried on during the past year was commenced by an investigation into the plasticity of a series of minerals, including a number of the chief rock-making constituents. These were for the most part the minerals constituting Mohs' scale of hardness. In this work the method suggested many years ago by Professor Kick was employed, which consists of submitting the material to differential pressure obtained by embedding the specimen in alum or sulphur, inclosing the whole in a stout copper box, and then slowly deforming it in a powerful press.

It was found that under these conditions rock salt, selenite, Iceland spar, and fluorite could be readily deformed. The next higher mineral on the scale of hardness—namely, apatite—also showed distinct evidence of plasticity, although this was much less marked than in the case of the minerals already mentioned. Diopside, when treated in this way, developed a most remarkably perfect twinning parallel to the base. This twinning is often seen in this species when it is found in the ancient crystalline schists, but has never formerly been produced in anything like the same perfection as in these experiments. The harder minerals (pyrite, garnet, and quartz) showed no plastic deformation, but were crushed to powder under the conditions of the experiment.

The flow of marble was then made the subject of further investigation, the experimental conditions being varied and the rock being subjected to much higher pressure than in former trials. One interesting result attained in this connection was the complete plastic deformation of this rock at ordinary temperatures, the constituent grains of calcite moving on their gliding planes without the development of any breaking or granulation. In former experiments this had only been accomplished when the rock was deformed at a temperature of at least 300° C.

The investigations were then extended to a series of impure limestones, presenting a great variety in character, some of them containing a large amount of clay, some highly arenaceous, and some bituminous. These were deformed in heavy tubes of nickel steel, both at ordinary temperatures and when heated to 300° or 400° C.

Experiments were also carried on with several typical dolomites, and it was found that while these could be made to flow, they did so much less readily than ordinary limestones.

The actual amount of pressure required to deform rocks and the amount of internal friction which they developed was also studied, four typical rocks being selected for this purpose, namely, Carrara marble, Coxeyville dolomite, "Belgian block," and Baveno granite.

As the cubic compressibility of rocks is a property which has an intimate bearing on rock flow, a series of determinations of this compressibility was made in the case of 16 typical rocks, including granites, various basic eruptives, limestones, etc. Apparatus has also been installed for the purpose of extending this experimental work on rock deformation to the harder crystalline rocks, and a study of the deportment of granite, gabbro, etc., under conditions of very high temperature and pressure is now about to be made.

**G. K. Gilbert,** Washington, D. C. Grant No. 126. *For preparing plans for investigating subterranean temperatures.* $1,000.

*Abstract of Report.*—Dr. Gilbert has considered the question of the best locality for drilling a deep well in rock of a uniform character, and reports that he has investigated the question of a suitable site (*a*) by formulating the conditions to be satisfied; (*b*) by a series of inquiries and consultations with geologists familiar with the structure of various districts east of the Great Plains; (*c*) by a personal visit to the district which appeared from description most likely to afford a satisfactory site. As a result of this investigation he reports that the Lithonia district, Georgia, both appears preferable to all other districts of which he has secured information and does in fact well satisfy the conditions requisite for a successful boring. No effort was made to choose a precise spot, but the natural conditions are there favorable over so large an area that the selection of a particular spot can be made in view of local economic conditions.

A reliable well-boring company has furnished an estimate and is willing to guarantee the completion of a well 6,000 feet in depth. Dr. Gilbert recommends that in view of the importance of such a project it be undertaken. The reasons given by him are printed in the papers accompanying this report, pages 259–267.

## HISTORICAL RESEARCH.

**Annie Heloise Abel**, New Haven, Conn. Grant No. 191. *For investigating the early Indian policy of the United States.* $150.

*Abstract of Report.*—The purpose of the investigation, as first undertaken, was to determine the causes and methods of moving the Indians from the eastern to the western side of the Mississippi.

The material available in Washington has been located and found to be so vast in amount that it has been deemed advisable to confine the investigation to the period preceding 1830. Most of the material is in the Indian Office, although the Jackson papers are particularly valuable, and about half the time—six weeks—has been spent in their perusal.

**William Wirt Howe**, New Orleans, La. Grant No. 199. *For preliminary inquiry into the subject of an investigation on legal history and comparative jurisprudence.* $1,000.

*Abstract of Report.*—The report suggests that a beginning of research may be made by taking up and comparing the codes of private law which have been adopted in the Americas and have been derived from French and Spanish sources, and thus relate back to Roman law. Fifteen such codes are mentioned, namely, those of Haiti, Bolivia, Peru, Chile, Lower Canada (Quebec), Nicaragua, Louisiana (revised), Guatemala, Argentina, Mexico, Ecuador, Spain (extended to Porto Rico and the Philippines, as well as to Cuba), Colombia, Brazil, and Uruguay. The method of comparison and contrast adopted by M. de St. Joseph in his Concordance of Continental and Other Codes, Paris, 1840, is recommended; but it is deemed better to begin the work by a comparison of the four principal codes in North America in the list above detailed, namely, those of Lower Canada, Louisiana, Mexico, and Spain, the last being fundamental in Porto Rico, Cuba, and the Philippines. They should be rendered into English, printed in parallel columns, and annotated with explanatory references to Roman law and to such judicial decisions as may best interpret the meaning of their provisions.

## MATHEMATICS.

**Derrick N. Lehmer**, Berkeley, Cal. Grant No. 190. *For pay of assistants to make the entries in a table of smallest divisors.* $500.

*Abstract of Report.*—Since receiving the grant, Professor Lehmer has had one assistant constantly at work. All but about 150,000 of the entries are now in, or the table of factors is about 90 per cent completed, so far as the making of entries is concerned; but the remaining work will be slower, and it is difficult to foretell how long it will take for completion.

This work will contain in one volume the prime factors of all numbers from one to ten million. Similar tables up to the tenth million have been published at various times, but they are generally

inaccessible, and scattered through several volumes. The tenth volume has never been published heretofore. The work is therefore an improvement and extension of existing tables.

**E. J. Wilczynski,** Berkeley, Cal. Grant No. 135. *For investigation of ruled surfaces, etc.* (Dr. Wilczynski is a research associate of the Carnegie Institution.) $1,800.

*Abstract of Report.*—As the results of Professor Wilczynski's work either have been published in the mathematical journals or else are to appear shortly, it seems unnecessary to give any detailed account of them. The following list gives the titles and places of publication:

1. A fundamental theorem in the theory of ruled surfaces. Mathematische Annalen, vol. 58, pp. 249–256.
2. Studies in the general theory of ruled surfaces. Trans. Am. Math. Soc., vol. 5, pp. 226–252.
3. Invariants of a system of linear partial differential equations and the theory of congenences of rays. To appear in Am. Jour. of Math., October, 1904 (36 pages).
4. On ruled surfaces whose flecnode curve intersects every generator in two coincident points. To appear in Trans. Am. Math. Soc., October, 1904 (6 pages).
5. General theory of curves on ruled surfaces. Offered to Trans. Am. Math. Soc. (about 15 pages).
6. General projective theory of space curves. Offered to Trans. Am. Math. Soc. (about 40 pages).
7. The general projective theory of space curves and ruled surfaces. Read at the Heidelberg International Congress of Mathematicians, and to be printed in its proceedings (about 6 pages).

One remark of a general nature may properly be made here. The general character of these investigations places them at the beginning of a new kind of geometry, a projective geometry, which does not confine itself to the consideration of algebraic cases, as has hitherto been the case, but which proves theorems of a more general nature by the use of differential equations, resembling in that respect the general theory of surfaces. It differs from this latter theory, however, in being a projective and not a metrical theory. In this general, projective, infinitesimal geometry, the theory of curves and ruled surfaces are merely the first chapters. The larger field promises to be of absorbing interest and importance.

### PALEONTOLOGY.

**Oliver P. Hay,** American Museum of Natural History, New York, N. Y. Grant No. 118. *For monographing the fossil chelonia of North America.* (For first report see Year Book No. 2, p. xxxvii.) $3,000.

*Abstract of Report.*—The work of monographing the fossil turtles of North America has been diligently pursued during the present

year (1904) and is now nearing completion. Most of the types of the described species have been examined, most of them refigured, and much new material has been studied. Free access to the collections at Washington, New York, New Haven, Cambridge, Princeton, Pittsburg, Chicago, Lawrence (in Kansas), and other cities, has been granted and enjoyed. Through the coöperation of the Carnegie Institution with the American Museum of Natural History, the writer was enabled to spend seven weeks of the summer of 1903 in the Bridger deposits of southwestern Wyoming. A large number of specimens of fossil turtles was secured, and these will throw much light not only on species and genera, based on fragmentary material, but also on questions of morphology and phylogeny. Besides the manuscript, there have been prepared over 300 drawings and about 125 photographs to illustrate the characters and the anatomy of the various species.

Use has been made of the opportunity to visit the principal museums of the continent and of England for the purpose of studying their chelonian materials and obtaining clear views regarding the relationship of the European genera to that of North America. All the museums visited have been freely opened to Dr. Hay.

**G. R. Wieland,** Yale University, New Haven, Conn. Grant No. 119. *For continuation of researches on living and fossil cycads, and illustration of memoir on the structure of the latter.* (For first report see Year Book No. 2, p. xxxvii.) $2,300.

*Abstract of Report.*—The further studies of the cycads and their illustrations have been carried forward by Dr. Wieland during the year along the lines originally proposed, namely, a first or botanical and a second or taxonomic investigation. The results of the more strictly introductory or structural study have been brought together in an extended illustrated memoir, which will be ready to go to press in the near future. This memoir treats mainly of the general habits of growth, and the vegetative and reproductive structures of the silicified cycadean stems from the lower Cretaceous and upper Jurassic of South Dakota and Wyoming. As is now well known from the preliminary papers already published by Dr. Wieland, these cycads present structures of the most fundamental importance in our conception of plant morphology and evolution. Their wonderful preservation and the greatly improved methods of section cutting noted in the report of last year have made possible a study more complete perhaps than in the case of any other extinct group of

plants. Aside from the Marattiaceous structure of the synangia, the most important single determination made is that the strobili of some of the Bennettiteæ were functionally bisporangiate or bisexual, a condition foreshadowed by *Tumboa* as having earlier existed among the gymnosperms, but never before demonstrated in any member of the group. These features bring the gymnosperms into close apposition to the angiosperms, and strongly suggest a derivation of both series of seed-bearing plants from a filicinian ancestry.

PHYSICS.

**S. J. Barnett,** Stanford University, Cal. Grant No. 149. *For research on the electric displacement induced in a certain dielectric by motion in a magnetic field.* $250.

None of the experimental work planned by Professor Barnett has yet been undertaken, as the necessary apparatus is still in process of construction.

**William Campbell,** Columbia University, New York, N. Y. Grant No. 179. *For research on the heat treatment of some high-carbon steels.* $1,500.

*Abstract of Report.*—A series of high-carbon steels were heated to temperatures varying from 650° to 1,200° C. and slowly cooled. Their mechanical properties have been worked out, their electrical conductivity has been measured, and a preliminary examination of their microstructure made.

The work will be continued by a detailed examination of their microstructure. This will be followed by a series of experiments on quenching and tempering, and the structure of the hardened steels will be worked out, in connection with their transformation points.

**H. S. Carhart,** University of Michigan, Ann Arbor, Mich. Grant No. 151. *For preparation of material for standard cells, etc.* $500.

*Abstract of Report.*—The problem to be solved is the determination in absolute measure of the electromotive force of Clark & Weston standard cells, both of which are used as standards of electromotive force in all the civilized countries of the world.

An uncertainty of about one part in 1,000 exists in the value of the electromotive force of these cells. The legal value for the Clark cell in the United States is 1.434 international volts at 15° C., but measurements made by Professor Carhart and Dr. Guthe (now of the Bureau of Standards) in 1899, as well as those made since by indirect methods in other parts of the world, show that the true

value is probably nearer 1.433 than 1.434. A similar uncertainty exists relative to the Weston normal cell.

To make the proposed determination it is necessary to design and construct some form of electrodynamometer or ampere balance to measure currents in terms of centimeters, grammes, and seconds. The work referred to in 1899 was done with an imperfect instrument, but the success attained was such as to warrant the construction of a better electrodynamometer with greater refinements of detail, construction, and measurements. This has been done in conjunction with one of Professor Carhart's colleagues, Prof. George W. Patterson, without whose assistance, particularly in the mathematical solution of the electromagnetic action of one coil on another and the resultant torque, the work would have been almost fruitless. They have constructed a large electrodynamometer composed of one stationary and one movable coil. Both coils are wound on cylinders made of plaster of Paris, accurately turned and covered with a thin coating of shellac. The large coil has a winding of 593 turns of silk-covered copper wire, occupying a length of about 41 cm., and the cylinder has a diameter of 47 cm. The relation between length and diameter was intended to be as nearly as possible $\sqrt{3}$ to 2. The same relation holds for the inner suspended coil, which has a diameter slightly over 10 cm. For the suspension both phosphor-bronze and steel wires have been experimented with. The principle of the instrument is the balancing of the torque, due to the electromagnetic action between the two coils against the torque of the suspending wire twisted through 360°. A twist of one complete turn was chosen, because mirrors at the two ends of the wire permit a complete turn to be measured with the greatest accuracy by means of two telescopes and scales.

The couple required to twist the suspending wire through one turn is determined by separate observations on the period of torsional vibration with a load whose moment of inertia can be computed with great accuracy. The design of the instrument is such that approximately one ampere is required to produce a balance.

The current thus measured is carried through a standard ohm, and the difference of potential between its terminals is then compared with the electromotive force of the standard cell by means of an accurately adjusted Wolff's potentiometer.

About one hundred standard cells are available for the measurement. The chief difficulty encountered up to the present is the elastic fatigue of the suspending wire. In all the wires tested thus

far this fatigue exceeds the limits which they have set as affecting the accuracy aimed at. The immediate improvement in the apparatus contemplated is the lengthening of the tube carrying the torsion head, so as to use a suspending wire two meters long instead of one a little over one meter, as at present. This change, coupled with a decrease in the weight of the suspended system, will diminish the elastic fatigue or set. They see no insurmountable obstacle to complete success, but find that much time is consumed in the preliminary work before satisfactory and trustworthy results can be obtained.

A preliminary report of the work already done was given at the International Electrical Congress in St. Louis. It is hoped that the work may be completed during the academic year 1904–1905.

**C. D. Child**, Colgate University, Hamilton, N. Y. Grant No. 194. *For investigation of the ionization in the neighborhood of a mercury arc in a vacuum.* $50.

*Abstract of Report.*—A few measurements were made of the discharge from an iron electrode to the arc which was formed in a vacuum between mercury terminals. Further measurements are to be made varying the distance and the potential difference between the electrode and the arc. From this it is hoped that the velocity of the ions may be computed.

Measurements have also been made of the drop in potential at the anode and that at the cathode and the total potential difference around the arc with mercury, carbon, graphite, iron, and copper electrodes in a vacuum, with various combinations of these in a vacuum, and with carbon, graphite, and iron in hydrogen. Some experiments were also made using an alternating E. M. F.

**Henry Crew**, Evanston, Ill. Grant No. 10. *For study of certain arc spectra.* (For first report see Year Book No. 2, p. xxxviii.) $1,000.

1. Concerning the preparation of photographic spectrum map of the metallic arc, Dr. Crew sent to the Institution a map of the aluminium arc which was completed shortly after his last report. In the preparation of this map two new $Al_2O_3$ flutings were discovered. During the coming year he hopes to complete a map of the mercury arc, using the same apparatus.

2. Concerning the E. M. F. of the intermittent metallic arc, the oscillograph made by the Cambridge Scientific Instrument Company enabled him to determine these (E. M. F.) curves very satisfactorily.

The results of this work are embodied in a paper "On the conditions which govern the appearance of spark lines in arc spectra."

Dr. Crew makes the comment on this paper that, in addition to the solution of the original problem, it contains also the explanation of the hitherto anomalous fact that an atmosphere of hydrogen introduces spark lines into arc spectra. Both the phenomenon and the explanation may be of considerable importance in astrophysics.

3. As to the third problem, namely, to find the order, in point of time, in which the lines of Mg and zinc make their appearance, the situation has not changed since the last report, when it was stated that unexpected difficulties arose when the attempt was made to pass from the carbon spark to the metallic spark. In fact, the metallic spark cools down so quickly that the entire phenomenon is over in something like one one-thousandth of a second.

**George E. Hale,** Mount Wilson, Cal. Grant No. 152. *For experiments on the use of fused quartz for the construction of optical mirrors.* $3,000.

The recent developments of astrophysical research have shown the necessity of constructing horizontal reflecting telescopes of great focal length, especially for photographic observations of the sun. The most serious difficulty in accomplishing this appears to lie in the fact that the form of the mirrors employed in the coelostat telescope changes through the expansion caused by the sun's heat. This tends to injure the definition of the solar image, and thus to prevent the accomplishment of the highest class of work.

In 1903 Dr. Elihu Thompson suggested that if the mirrors could be made of fused quartz the difficulty should practically disappear, since the expansion of fused quartz by heat is only about one-tenth that of glass. A grant made by the Carnegie Institution permitted experiments in this direction to be undertaken, with the advice and coöperation of Dr. Thompson. The immediate supervision of the work was intrusted to Prof. G. W. Ritchey, superintendent of instrument construction at the Yerkes Observatory. After it had been decided to erect the Snow telescope on Mount Wilson, it became necessary for Professor Ritchey to accompany the expedition to California, in order that he might take charge of the construction of the new instruments required in the investigation. It was therefore decided to make the quartz experiments in Pasadena, where the Edison Electric Company kindly offered suitable space in its power-

house. After consultation with Dr. Thompson, who had made important preliminary experiments with fused quartz at Lynn, Professor Ritchey was fortunate enough to secure the assistance of Mr. Acheson, of the Acheson Graphite Company, and Mr. Tone, of the Carborundum Company, at Niagara Falls, in designing a special electric furnace for fusing the quartz. This is now under construction at Pasadena. A 50-kilowatt transformer, giving from 15 to 30 volts, has been completed, and an optical pyrometer for the measurement of the temperature of the fused quartz has been kindly loaned by Dr. S. W. Stratton, Director of the Bureau of Standards.

**E. Percival Lewis**, University of California, Berkeley, Cal. Grant No. 150. *To investigate vacuum-tube spectra of gases and vapors.* $500.

*Abstract of Report.*—This grant is to be expended mainly for quartz lenses and prisms for a large spectrograph, designed for a more systematic and detailed study of vacuum-tube spectra than has hitherto been made. A part of the necessary materials has been received, and it is expected that the spectrograph will be completed and in use in about two months. Meanwhile preliminary investigations have been carried on with a small spectrograph, the results of which are described in two papers published in the Astrophysical Journal for July, 1904.

**A. A. Michelson**, University of Chicago, Chicago, Ill. Grant No. 47. *For aid in ruling diffraction gratings.* $1,500.

*Abstract of Second Report.*—Professor Michelson continued his experiments during the year in connection with the building of ruling engines for diffraction gratings. He found many difficulties, and has not yet fully overcome all of them. The method employed for ruling is based essentially upon the construction of a precision screw. Professor Michelson believes that he can obtain results of greater value than have hitherto been reached by the development of a special engine that he is now working upon.

**R. W. Wood**, Johns Hopkins University, Baltimore, Md. Grant No. 120 *For research, chiefly on the theory of light.* (For first report see Year Book No. 2, p. xxxix.) $500.

*Anomalous Dispersion of Sodium Vapor.*—A very complete study has been made of the anomalous dispersion of the vapor of metallic

sodium, which has made possible an experimental verification of the simplest form of the electro-magnetic dispersion formula

$$n^2 = 1 + \frac{m\lambda^2}{\lambda^2 - \lambda_m^2}$$

This formula has never been tested, for the reason that no data have ever been obtained of the dispersion of a medium in which the velocity of light of different wave-lengths is dependent on the presence of a *single* absorption band. The dispersion of the vapor was measured by observing the shifts of the interference fringes in a Michelson interferometer when a given quantity of the vapor was introduced into one of the optical paths of the instrument. Usually two sources of monochromatic light were used simultaneously. When working close to the absorption band it was necessary to have lights of very nearly the same wave-length, which was accomplished by placing a helium tube in a powerful magnetic field and utilizing the resulting Zeeman double-line for illuminating the interferometer. Absolute determinations were made of the refractive index of the vapor formed in highly exhausted tubes of steel and porcelain at different temperatures, the temperature being determined by means of a thermo-couple of iron and constantin.

For light of wave-lengths very nearly that of the D lines the refractive index of the vapor at a temperature of 650° C. was found to be 1.38 for the wave-length on the red side of the absorption band and 0.62 for light on the blue side.

Numerical values were obtained for the refractive index from the extreme red to the remote ultra-violet, and the observed values were compared with the values calculated from the dispersion formula, most excellent agreement being found.

The vapor was found to have some very remarkable physical properties, which are at the present time under investigation. It appears to have the property of cohesion and perhaps surface tension. A dense mass of it can be formed in the center of a highly exhausted tube, bounded at each end by a vacuum, there being only a very slight amount of diffusion toward the colder parts of the tube.

The results of the work appear in the Proceedings of the American Academy and the Philosophical Magazine for September, 1904.

*The Fluorescence of Sodium Vapor.*—The work which was commenced in the spring of 1903 on the remarkable fluorescence of sodium vapor was continued during the following autumn. It was found practicable to photograph the fluorescence spectrum of the

vapor when illuminated with approximately monochromatic light, and some very remarkable relations between the wave-lengths of the absorbed and emitted radiation were found, which, it is believed, will eventually throw a great deal of light on the problem of fluorescence, for which we have at the present time no satisfactory theory. The work was suspended early in December, owing to the insufficient power of the spectroscope employed, but will be renewed again as soon as suitable apparatus can be constructed.

## PHYSIOLOGY.

**W. O. Atwater**, Wesleyan University, Middletown, Conn. Grants Nos. 134, 139, and 195. *For investigations in nutrition.* (For first report see Year Book No. 2, p. xxxix.) $7,000.

*Abstract of Report.*—The purpose of this grant was to promote research involving the direct determination of the amount of oxygen consumed by man for sustaining bodily functions. To this end a considerable portion of the fund was devoted to the development of an apparatus and method for determining the amount of oxygen in connection with the respiration calorimeter already in use.

Between October 1, 1903, and January 1, 1904, the work was continued and frequent tests of the efficiency of the apparatus were made. In addition to these, a very successful experiment with man was completed. The work of the year was thus more satisfactory in respect to both the development of the apparatus and method and the experiments actually accomplished with men than had been anticipated at the beginning.

For the year 1904 three grants have been made—No. 134 of $1,000, No. 139 of $4,000, and No. 195 of $2,000—of which the first two were for the continuation of the work already begun and the last was intended more especially for experiments in fasting. The work under these grants is still in progress.

Despite some exceptional difficulties, a number of very successful experiments have been carried out since January 1. These have included:

(1) General metabolism experiments with men, in which the effects of muscular work have been studied.

(2) A number of shorter and less complete experiments of approximately 12 hours' duration with several men to determine the heat emission and oxygen consumption, as well as the elimination of carbon dioxide and water under varying conditions of bodily position, muscular work, and amount of clothing.

(3) Experiments on metabolism during fasting. These have already been made with two different men during periods of two and three days, and have brought interesting results. We are now endeavoring to find a person who can comfortably endure a much longer period of fasting and who will serve as a proper subject for a systematic series of experiments.

The apparatus and method are proving very satisfactory for these inquiries. As is natural in the development of a new apparatus and method, difficulties arise from time to time and means are constantly being suggested for improvement.

By invitation, a description of the apparatus in its present form was given by Dr. Atwater at the late meetings of the British Association for the Advancement of Science in Cambridge, England, and of the International Physiological Congress in Brussels, Belgium, in August and September of 1904. These descriptions were illustrated by a small brochure, which gives summaries of the results of two experiments, including a final balance sheet of income and outgo of material and energy. It is of interest to note that these are the first instances in which a complete and accurate balance of this character has been made by actual experiment.

A detailed description of the apparatus in its present form, with experiments sufficient to illustrate the method of its use, is now being prepared for publication by the Carnegie Institution.

**Russell H. Chittenden,** Sheffield Scientific School of Yale University, New Haven, Conn. Grant No 197. *For a study of the minimal proteid requirement of the healthy man.* $1,500.

*Abstract of Report.*—The grant made for the study of this problem has been used in connection with grants from other sources for the experimental study of the possibilities of physiological economy in nutrition, with special reference to the proteid foods. The experiments have been conducted on three distinct types or classes of individuals: (1) A group of five men, of varying ages, connected with the university as professors and instructors—representatives of the mental worker rather than the physical worker; (2) a detail of thirteen men, volunteers from the Hospital Corps of the United States Army and representatives of the moderate worker; (3) a group of eight young men, students in the university, all thoroughly trained athletes, and some with exceptional records in athletic events.

In the conduct of the experiments it was recognized that while previous experimenters have shown the possibility of maintaining

body equilibrium and nitrogen equilibrium on a low proteid diet for a brief period, it is necessary, in order to have the results of physiological value, for the experiments to be conducted not simply for a few days or weeks, but through months and years. Consequently the experiments, which are now concluded, have extended with some individuals over a year, and all have covered at least six months of time.

The results obtained with these twenty-six individuals all agree in showing that there is no justifiable ground for the assumption that an adult man of average body-weight needs 118 grams of proteid food for the maintenance of health, strength, and vigor. On the contrary, it has been clearly demonstrated that it is quite possible to maintain body-weight and to preserve nitrogen equilibrium with an amount of proteid food per day equal to not more than 50 per cent that called for by the ordinarily accepted dietary standards.

Further, the experiments have clearly demonstrated that this condition of nitrogen equilibrium can be maintained without increasing the amount of non-nitrogenous food consumed daily. An average intake of 7 to 9 grams of nitrogen per day, with a total fuel value of the food amounting to 2,500 to 2,800 calories, was found quite sufficient to maintain body-weight and nitrogen equilibrium. In other words, a metabolism of less than 50 grams of proteid per day was quite sufficient for the needs of the body. In some cases even smaller quantities of proteid food sufficed to meet all the physiological requirements of the individual. The experiments also showed that with this low nitrogen intake there was a marked gain in bodily strength, as indicated by appropriate dynamometer tests. Further, the condition of the blood as regards the number of erythrocytes, leucocytes, and hæmoglobin-content was not altered by the low nitrogen intake. Moreover, there was no loss of mental vigor or change in reaction time.

All the details of the experiments, together with the various data and conclusions, are embodied in a report now in type, making a volume of about 500 pages, which will be ready for distribution within a few weeks.

**Arthur Gamgee**, Martreux, Switzerland. Grant No. 62. *For preparing a report on the physiology of nutrition.* $6,500.

No report received.

**Hideyo Noguchi**, University of Pennsylvania, Philadelphia, Pa. Grant No. 94. *For continuation of the studies on snake venoms.* $1,700.

*Abstract of Report.*—Dr. Noguchi continued his studies on snake venoms, upon which he has been engaged since 1900. Under the present grant he has succeeded in preparing the antivenins for the *Crotalus adamanteus* and water-moccasin venoms. The production of the anti-moccasin venin was thus for the first time attempted and accomplished, while the anti-crotalus venin had already been prepared by Flexner & Noguchi about a year ago. With the above-named two antivenins several series of therapeutic experiments have been performed. The results of these experiments show a very high therapeutic value of the antivenins, as being able to save the life of animals inoculated with certain multiple lethal doses of corresponding venom, even when the symptoms were critically advanced. It has been a common belief that an antivenin prepared with one kind of venom can counteract the poisonous effects caused by the other kinds of venom, irrespective of the source of the venom. This unitary view of the nature of antivenin has lately been the point of much discussion, and many experimental evidences have been brought up against it. Dr. Noguchi, having had the opportunity of utilizing several kinds of antivenins for this purpose, has tested each antivenin against different sorts of snake venom. The results obtained by him prove conclusively that different antivenins act highly, if not absolutely, specific, both in the animal body and in vitro, to the venoms through which they are produced. From this fact he concludes that in treating the snake bites only the specific antivenins are to be employed.

Since Flexner & Noguchi discovered the fact that the hæmolytic principles of snake venoms require certain secondary substances in order to complete their "laking" action, attention was directed to this phenomenon by some investigators, and Kyes has finally succeeded in discovering the rôles played by lecithin in the venom-hæmolysis. Dr. Noguchi made a routine examination over a considerable number of acids and salts concerning the so-called venom-activating properties of these chemically definite substances, and has found that there are, besides lecithin and kephalin, still many substances which are able to produce hæmolysis upon the blood corpuscles previously treated with snake venom, even when present in such small amount as to remain entirely without hæmolytic effect upon

the non-venomized corpuscles. A number of high acrylic acids and their salts, as well as a few high normal fatty acids, possess the so-called venom-activating properties. A group of experiments, both in the animal body and in vitro, concerning the neutralization of snake venoms and antivenins have been made. The experiments under this topic have to deal with the questions on the nature of the neutralization curves of toxin and antitoxin from the physico-chemical side of view. Similar experiments have also been made with saponin and cholesterin. The velocity of reaction at different temperatures of acids and venoms (upon blood corpuscles) has been determined. The relation between the susceptibility of animals and their body-weight has been studied.

The above-stated work has been carried out at the Statens Serum Institut, Copenhagen, during a period extending from October, 1903, to September, 1904. The work has already been partly published and the rest soon will be.

**Edward T. Reichert** and **Amos P. Brown**, University of Pennsylvania, Philadelphia, Pa. Grant No. 188. *For research on the crystallography of hæmoglobin.* $1,000.

*Abstract of Report.*—As this grant was not made until April, little progress could be made after June 1 on the preparation of crystals, owing to the warm weather. About five weeks of satisfactory work was done. In this period Drs. Reichert and Brown prepared and examined crystals from the blood of 18 different animals and obtained very satisfactory results in regard to their crystallization. The list includes fishes, batrachians, reptiles, birds, and mammals. It would be possible, with the data collected, to distinguish accurately between the bloods of all of the species thus far examined. With the arrival of cooler weather work is beginning again, and they expect to make rapid progress with the investigation during the winter.

ZOÖLOGY.

**A. J. Carlson**, Stanford University, Cal. Grant No. 196. *For research on the physiology of the invertebrate heart.* $100.

Mr. Carlson received a grant as a research assistant in 1903. His report covers the work of 1903 and 1904.

*Abstract of Report.*—The molluscan and the arthropod (crustaceans, Limulus) heart is provided with regulative nerves. In the crustaceans these nerves take their origin from the thoracic ganglion; in Limulus they arise from the brain and the abdominal ganglia; in

the chitons, from pleural nerve-cords; in lamellibranchs and marine gasteropods, from the visceral ganglion or ganglia; in pulmonates and cephalopods, from the subœsophageal ganglion. In the gasteropods the nerve-fibers enter the heart both at the auricular and at the aortic ends.

The arthropod heart is supplied with both inhibitory and accelerator fibers, the latter coming from the central nervous system anterior to the former, a condition similar to that in the vertebrates. The cardiac nerves of the lower gasteropods (chitons, prosobranchs, tectibranchs) appear to be only accelerator in function. In the nudibranchs and the pulmonates both inhibitor and accelerator cardiac nerves are present. In the lamellibranchs and the cephalopods the main, if not the sole, function of the nerves is inhibitory.

In Limulus the heart-muscle does not possess automaticity. The heart-beat is neurogenic, or due to the activity of the ganglion cells on the dorsal surface of the heart. There is some evidence that the heart-beat in the other invertebrates is also neurogenic.

In Limulus the coördination or conduction in the heart takes place in the nervous and not in the muscular tissues.

In Limulus the cardio-inhibitory nerves act on the ganglion cells in the heart and not directly on the heart-muscle.

The arthropod, the molluscan, and the tunicate heart exhibit no refractory period, but the excitability is lowest at beginning of systole. The amplitude of contraction varies with the strength of the stimulus. The heart can be tetanized.

Single induced shocks, as well as the interrupted current of a certain intensity sent directly through the arthropod, the molluscan, and the tunicate heart, produce inhibition of the rhythm, partial or complete. This inhibition is due (1) to the stimulation of inhibitory nerve-endings in the heart, (2) to direct action of the electrical current on the rhythmical tissue. In Limulus this direct action of the current is on the automatic ganglion cells and not on the muscle, and this is probably true of the other invertebrates. This action of the induced current on the ganglion cells is probably of the nature of overstimulation or "shock."

Solutions of curare, atropin, and nicotin of sufficient strength to affect the heart accelerate the rhythm; strong solutions produce tetanus or "tonus" contractions.

These alkaloids paralyze (at least temporarily) the inhibitory nerves in the heart, but not the accelerator or motor nerves.

In Limulus the accelerator action of these drugs is on the ganglion

cells and not on the muscle. This is probably true of their action in the other invertebrates.

**W. E. Castle** and **E. L. Mark**, Museum of Comparative Zoölogy, Cambridge, Mass. Grant No. 136. *For experimental studies in heredity.* $500.

The work of Drs. Castle and Mark is in coöperation with the Station for Experimental Evolution at Cold Spring Harbor.

*Abstract of Report of W. E. Castle, 1904.*—Observations made on about 3,000 guinea-pigs and 200 rabbits, whose ancestry is known in most cases for several generations, indicate that:

(1) There occur in guinea-pigs at least three different pairs of alternative coat-characters, which conform closely to Mendel's law of heredity. These are pigmented coat, which dominates over albino coat; short or normal coat, which dominates over long or angora coat, and rough or rosetted coat, which dominates over smooth coat. These three pairs of characters are independent one of another in transmission. Two of them occur in rabbits, as well as in guinea-pigs, and are transmitted in the same manner as in guinea-pigs.

(2) In crosses between two different types of albino rabbits, Himalayan and pure white, dominance of the Himalayan type is imperfect, but segregation of the two types in the next generation is complete.

(3) In crosses between lop-eared and normal rabbits an intermediate condition is obtained, which persists without segregation in the next generation. In other words, this seems to be a case of non-Mendelian, but of blended inheritance.

(4) Latency is a phenomenon entirely distinct from recessiveness. It is the condition of a dominant character when present unseen in a recessive individual or germ. The presence of the dominant character may be demonstrated by cross-breeding.

A full discussion of these topics may be found in a paper now in course of publication. Data for the study of the laws of transmission of several other characters have been accumulated.

**Henry E. Crampton**, Columbia University, New York, N. Y. Grant No. 137. *For determining the laws of variation and inheritance of certain lepidoptera.* (For first report see Year Book No. 2, p. xli.) $500.

*Abstract of Report.*—During the year more than a thousand pupæ have been statistically examined, and over five hundred emerging moths have been paired in order to obtain data bearing upon the

problem of sexual selection. The forms used most extensively were *Philosamia cyanthia* and *Rothschildia jorulla*, a Mexican species, additional data being obtained from *Hyperchiria io*, *H. budleyi*, *Rothschildia orizaba*, and *Samia ruber*. Studies upon the course of inheritance in these species have also been prosecuted, the second and third generations being obtained in some cases. Owing to the peculiar nature of the material, it is impossible to present an extended report upon the results obtained at the present time.

**J. E. Duerden**, University of Michigan, Ann Arbor, Mich. Grant No. 158. *For continuation of investigation on the morphology and development of recent and fossil corals.* $1,500.

*Abstract of Report—Fossil Corals.*—Investigations have been carried out upon a large series of palæozoic fossil corals obtained last year from various collections at home and abroad. The studies, conducted along developmental lines, have demonstrated conclusively (1) that the primary stage of the rugose coral is hexameral, in contrast to the tetrameral, which hitherto has been usually assumed; (2) that the later septa are added in a definite sequence within only four of the six primary chambers. The results have permitted discussion of the relationships of the Tetracoralla to other groups of Anthozoa, the conclusion being that they are most nearly related to the living zoanthid actinians. A paper has been already published, and another, "The Fossula in Rugose Corals," is submitted for publication.

*Recent Corals.*—Two papers devoted to the morphology of recent coral polyps have been already published during the year, and a third is almost ready for publication. This summer an expedition has been conducted to the Hawaiian Islands for the purpose of securing a series of Pacific corals for comparison with results already published upon West Indian forms. About three months were spent among the islands in the collection and study of the living corals. Between thirty and forty species were secured, and material preserved for later investigation, while over fifty cases of dried specimens were obtained for studies on variation. The collection includes many types not yet studied morphologically, and others which afford interesting comparison with West Indian types.

A series of experiments upon the physiology and reactions of living coral polyps were conducted, and important facts bearing upon their method of feeding were secured, demonstrating that the exudation of mucus plays an important part in the process.

A collection was made of the Hawaiian shallow-water actinians to

supplement the deep-sea forms obtained in 1902 by the U.S. Bureau of Fisheries, the writer having in hand the preparation of a report upon the group. Observations and experiments were also made upon the unique commensalism of certain crabs and actinians, the former carrying the latter in their claws and utilizing them for the purpose of securing food.

**Carl H. Eigenmann**, University of Indiana, Bloomington, Ind.
Grant No. 68. *For investigation of blind fishes in Cuba.* $1,000.

*Abstract of Report.*—In March, 1902, Professor Eigenmann made extensive collections in the caves of western Cuba, and secured, among others, one female blind fish containing unborn young 20 mm. long, in which the eyes are remarkably well developed.

In order to determine the breeding season and to obtain early embryos of the blind fishes Lucifuga and Stygicola, he spent parts of October and November and December of 1903 and August and September of 1904 in Cuba. Large numbers of adult fishes were taken, and many more could have been secured. But it was found that while occasional specimens containing young may be expected at any time of the year, the chief breeding season has so far been missed, and that these fishes probably breed in June and July, at the culmination of the wet season, when the height of water in the caves may make collecting difficult. The caves will be visited again in June and July of 1905.

Cages of fine wire screening, protected by strong screening, were built in one of the caves and stocked with fish. These cages proved worthless under the conditions existing in Cuba, and other plans will have to be tried to rear fishes in the light.

Several attempts were made to bring living fishes to Indiana with a view of possibly colonizing them in one of the Indiana caves. While a few specimens were brought through alive, the mortality *en route* and their extreme sensitiveness to cold puts the idea of colonizing them in northern caves out of court.

A monograph on the eyes of the fishes from birth (20 mm.) to old age will probably be finished during the winter.

**L. O. Howard**, Department of Agriculture, Washington, D. C.
Grant No. 122. *For preparing a report on American mosquitoes.*
(For first report see Year Book No. 2, p. xlii.) $2,500.

*Abstract of Report.*—Dr. Howard has submitted a full report of progress, from which it appears that the number of species of mos-

quitoes actually under study amounts to 94, of which the early stages of 65 have been observed and collected. The plan followed during 1903, of employing local observers, was during 1904 done away with to a large extent, only two such observers, one in Montana and the other on the southwest coast of Mexico, being employed. General collecting trips were made by two assistants, following the line of demarcation of the upper and lower austral zones from south Texas to Virginia, in the course of which many facts of importance were gathered regarding the northward distribution of the yellow-fever mosquito. Another trip was taken with a similar purpose into south Mexico, where the influence of altitude upon the distribution of this important species was carefully studied. The preparation of the illustrations for the monograph has been begun, and 37 species of adults and 23 species of larvæ have been drawn in admirable style. At the time of writing other drawings were under way, including a series indicating the anatomical details of the early stages. An enormous number of individuals of the different species have been received, and some very curious results have been obtained, indicating the presence in some cases of two or more distinct species indistinguishable by study of the adults alone, surprising larval differences indicating the fact.

**C. E. McClung**, Kansas University, Lawrence, Kans. Grant No. 16. *For making a comparative study of the spermatogenesis of insects, etc.* (For first report see Year Book No. 2, p. xliii.) $500.

*Abstract of Report.*—The second year's work by the holder of this grant has again been largely that of preparation of material for study. There is now on hand an extensive series of specimens which will make possible a comparative study of almost a hundred species of Orthoptera. A part of this material has been subjected to the action of radium and chemicals in the hope of producing some alteration of the chromosomes in division that would throw some light upon normal processes. No forms outside of the insects, in which hybrids could be obtained, exhibited satisfactory chromosomes, and so far it has not been found possible to secure the desired hybridization of insects, so that this most important part of the investigation will have to be postponed. The present work will be devoted to the study of two closely related species of one genus in which there are strongly marked chromosomes in the hope of determining some relation between the individual chromosomes and body characters. In connection with this two closely related genera will receive a similar

treatment, as will also two widely removed genera in a subfamily. If these investigations indicate the possibility of connecting certain chromosomes with definite groups of characters, efforts will be made later to carry out the difficult task of hybridizing the species that offer the best material for study.

**William Patten**, Hanover, N. H. Grant No. 157. *For studies relating to the origin of vertebrates.* $500.

*Abstract of Report.*—By means of the grant to aid in procuring material for the study of the origin of vertebrates, many Devonian fishes were obtained from New Brunswick.

The specimens of *Bothriolepis canadensis* were more perfect than any others that have ever been found. They will furnish the necessary data for a complete restoration in great detail of a typical representative of the Ostracoderms, one of the oldest and most primitive subdivisions of the Chordata known.

The structural features shown by this new material will necessitate the removal of the Ostracoderms from the Agnatha, separate them farther than ever from the true fishes, and will raise them to the rank of a new and independent class.

**Raymond Pearl**, University of Michigan, Ann Arbor, Mich. Grant No. 125. *For an investigation by statistical methods of correlation in variation.* $500.

*Abstract of Report.*—The grant was expended as follows: (*a*) In the purchase of calculating machines, measuring instruments, and other minor apparatus. (*b*) In procuring clerical assistance in the reduction of data. (*c*) In purchasing literature to which access could not otherwise be had.

During the year Dr. Pearl and students under his direction have been engaged in work along the following lines:

(1) The variation in the weight of the human brain and the correlation between this and other characters of the body. A paper on this subject has been completed and is submitted with the report.

(2) The effect of environmental changes of known quality and measured intensity on variation and correlation in the Protozoa.

(3) The correlation between the same and different characters in conjugating individuals of Paramecium (homogamy).

(4) The variation and correlation in certain of the component parts of the character "stature" in man.

(5) The correlation between differentiated homologous organs and undifferentiated homologous organs in the crayfish.

(6) The correlation between the death rates at different periods of life in man.

(7) Certain minor problems in variation and correlation.

A brief paper bearing the following title is submitted with the report: "A table to be used in calculating the probable error of the coefficient of variation."

This table will be of practical utility in biometrical investigation.

**W. L. Tower,** University of Chicago, Chicago, Ill. Grant No. 181.
*For an investigation of the potato beetles of Mexico.* $445.

*Abstract of Report.*—This grant was made to aid in the continuation of a research upon the evolution of the genus Leptinotarsa, a genus of beetle well calculated to give information concerning the causes and methods of evolution in insects. The work planned to be carried out under this grant falls under three heads: First, to trace more accurately the distribution of certain species of these beetles and to study the correlation of this distribution with topographic and meteorological conditions; second, to produce and transport to Chicago certain species and their food plants for purposes of experimentation; third, to carry on observations in the Mexican tropics on the life histories of these forms, and especially to study the factors most concerned in hibernation, and to start experiments in the transplantation of species from one habitat to another.

In order to carry out the investigation, Mr. Tower made two trips to Mexico—one at the close of the dry season and one at the beginning of the wet season. He determined important facts in relation to the distribution and hibernation of the beetles, and records of relative humidity, soil conditions, soil temperatures, air and sun temperatures, in order to further continue the experimental work in an intelligent manner. Living material of several species, together with their food plants, were brought to Chicago successfully, and have thrived well under the conditions provided for them. With this material experiments in breeding and hibernization and with various environmental factors will be continued.

Transplantations of various species were made from their native habitats into habitats entirely new to them. These, if successful, ought to give most valuable data concerning the effect of a new environment in the production of modifications and new species.

The results of this expedition consist in the obtaining of new and needed material and of information concerning environmental conditions during the rainy season.

With the material brought alive from Mexico, experiments in pedigree breeding, hibernizing, and experiments to determine the effect of temperature, moisture, etc., in the production of new characteristics in the species are being carried on at Chicago. These are progressing satisfactorily under excellent conditions, and bid fair to give desirable results in due time. In any such research it is only after prolonged study through generation after generation that results at all worthy of consideration can be obtained.

**H. V. Wilson**, University of North Carolina, Chapel Hill, N. C. Grant No. 33. *For morphology and classification of deep-sea sponges.* (For first report see Year Book No. 2, p. xliv.) $1,000.

With the aid of the grant Professor Wilson was enabled to spend fourteen months (July, 1902–August, 1903) in Europe engaged in the uninterrupted study of certain deep-sea sponges. These sponges formed part of a collection made in 1891 by the U. S. steamer *Albatross*, under the direction of Mr. Alexander Agassiz, in the Pacific Ocean, off the coasts of Mexico, Central and South America, and off the Galapagos Islands.

The bulk of his time abroad was spent in Berlin, where he occupied a working place in the laboratory of Prof. F. E. Schulze, the eminent authority on the classification of the Hexactinellida and on the structure of sponges at large. Professor Schulze's collections of Hexactinellid sponges are unrivaled. The collections of sponges in the adjoining Museum für Naturkunde, which are under the charge of Prof. W. Weltner, likewise proved most valuable. In the use of the collections, the libraries, and the photographic and other apparatus, every facility was allowed, both in the zoölogical laboratory and in the museum.

During the summer of 1903 he visited the Rijks Museum in Leyden, the Muséum d'Histoire Naturelle in Paris, and the British Museum of Natural History in London. In each museum every opportunity was allowed for the study of the types.

On his return to America Professor Wilson wrote up the results of his investigation. This work has just been published as one of the "Memoirs of the Museum of Comparative Zoölogy at Harvard College" (vol. xxx, No. 1. Reports on an Exploration off the West Coasts of Mexico, Central and South America, etc. xxx. The Sponges. By H. V. Wilson, pp. 1–164, with 26 plates).

*Abstract of Report.*—In addition to the discovery of new species, certain results of general biological interest accrued from the study

of the collection. Some remarkable forms were made known. Among such may be mentioned *Sclerothamnopsis compressa*, which resembles in the shrub-like habitus of its stony skeleton the hitherto unique *Sclerothamnus clausii* Marsh.

Light was thrown on the habitat of some of the Hexactinellida living at great depths. Thus *Caulophacus* was found growing upon the root spicules of *Hyalonema*.

Evidence of a convincing character was gained that the complex tubular hexactinellid genera *Eurete* and *Farrea* are derived ontogenetically from simple cup-like forms.

In several Hexactinellids what may be described as a peculiar hypertrophy of the skeleton was observed. The phenomenon is probably pathological and may indicate an effort of the sponge to shut off one part (diseased ?) of the body from the rest.

Observations were made on several aberrant forms of sponge spicules, with the result that more has been learned as to the phylogeny of such skeletal elements as the discohexasters and scopulæ of Hexactinellids and the protriænes and asters of Tetractinellids.

Our knowledge of the character and extent of variation in sponges has been increased by the study of this collection. Cases are recorded in which variation within a species affects not only the body shape, but the general anatomy as well. For instance, in a species of *Iophon* the character of the surface varies conspicuously, owing to divergence in the character of the main canals and the surrounding tissue.

Among the numerous variation phenomena exhibited by the skeleton, an excellent case of correlated variation was discovered in a species of the hexactinellid genus *Caulophacus*. Here the spicules coating the two opposite (pore and oscular) surfaces of the body vary in the same direction, and thus in different individuals the proportionate difference between them is preserved. In certain sponges the variation exhibited by the spicules tended toward the condition characteristic of a different though allied species or subspecies. A striking case was afforded by the new hexactinellid form *Farrea occa-claviformis*, in which some spicules were found closely similar to the highly specialized clavulæ characteristic of *Farrea convolvulus* F. E. Sch. Two cases of a phenomenon were found, which is perhaps to be regarded as a kind of qualitative variation. The phenomenon in question is briefly this: Two sets of individuals living together in the same locality and which are otherwise indistinguishable differ conspicuously in respect to a single point. One case was afforded by the monaxonid sponge *Iophon*, the other by the hexactinellid

*Eurete*. In both cases the point of difference was one involving the shape of a characteristic spicule.

**N. Yatsu**, Columbia University, New York. Grant No. 138. *For experimental studies of the Nemertine egg.* $300.

*Abstract of Report.*—Mr. Yatsu reports that he carried out, under this grant, series of experiments on the Nemertine eggs, during the summer of 1904, at the Harpswell Laboratory of Tufts College, South Harpswell, Me. The object of his work, which requires three or four summers, is to obtain a thorough knowledge of localization of the germinal regions of each stage of development, taking the egg of *Cerebratulus lacteus* as a type, and to find out, in the end, the initiating factor or factors of tissue differentiation. To this end, by removal and isolation experiments, he studied very successfully the morphogenic as well as cleavage factors in the early stages of development, and added several facts new to physiological embryology. He also used calcium-free water to modify the mode of cleavage. He actually demonstrated by crucial experiments the formation *de novo* of centricles in the egg-cytoplasm. This is a very important contribution to experimental cytology.

**Marine Biological Laboratory**, Woods Hole, Mass. J. Blakely Hoar, treasurer. Grant No. 123. *For maintenance of 20 tables.* (For first report see Year Book No. 2, p. xlv.) $10,000.

*Abstract of Report.*—As in the year 1903, the grant was made to aid the laboratory by paying for the maintenance of twenty research tables. The persons assigned to the tables were selected by the Carnegie Institution. The following seventeen persons occupied the Carnegie tables during the season of 1903:

Bryan, Walter, College of City of New York, August 3 to *after* September 12.
Carlson, Anton J., Stanford University, June 3 to September 5.
King, Cyrus Ambrose, DeWitt Clinton High School, July 7 to August 25.
Koch, Julius A., Western University of Pennsylvania, July 7 to August 10.
Kraemer, Henry, Philadelphia College of Pharmacy, July 12 to August 17.
Lewis, Warren H., Johns Hopkins University, June 27 to August 29.
Loeb, Leo, University of Pennsylvania, July 3 to September 3.
McClendon, J. F., University of Pennsylvania, July 13 to *after* September 12.
Minor, Marie L., Wadleigh High School, N. Y. City, July 14 to August 20.
Rhodes, Frederick A., Western University of Pennsylvania, July 7 to August 10.
Richardson, Harriet, Washington, D. C., June 17 to September 10.
Simons, Etoile B., The University of Chicago, June 30 to August 29.
Spaulding, Edward Gleason, College of City of New York, June 27 to August 11.
Streeter, George L., Johns Hopkins Medical School, July 2 to August 27.
Strong, R. M., The University of Chicago, July 30 to *after* September 12.
Treadwell, Aaron L., Vassar College, June 22 to *after* September 10.
Yerkes, R. M., Harvard College, August 15 to September 12.

The following, for various reasons, resigned their appointments:

Wallace Craig, University of Chicago, resigned June 6, 1904.
Dr. W. C. Curtis, University of Missouri, resigned June 29, 1904.
B. M. Duggan, University of Missouri.
J. A. Edquist, Gustavus Adolphus College, St. Paul, Minn.
W. F. Mercer, Ohio University, Athens, Ohio, resigned June 29, 1904.
Max W. Morse, Ohio State University, resigned June 6, 1904.
Porter E. Sargent, Cambridge, Mass.

The director of the laboratory, Dr. C. E. Whitman, sent the following statement of the investigators at the laboratory during the season of 1904; he also stated that the laboratory would have accommodations for a few investigators from October to May, or during the cold season. Forty-seven institutions were represented by investigators.

INVESTIGATORS.

| | |
|---|---|
| Zoölogy: | |
| Occupying rooms | 29 |
| Occupying tables | 3 |
| Physiology: | |
| Occupying rooms | 9 |
| Botany: | |
| Occupying rooms | 10 |
| | 51 |

**Naples Zoölogical Station**, Naples, Italy. Grant No. 124. *For maintenance of two tables.* $1,000.

*Abstract of Report.*—One of the tables was occupied by Dr. H. S. Jennings, now of the University of Pennsylvania, from September 1, 1903, to July 1, 1904; a second by Dr. Bradley M. Davis, of the University of Chicago, from February 29, 1904, to June 1, 1904. Dr. Edmund B. Wilson, of Columbian University, occupied a table from May 27, 1904, to July 14, 1904.

When not occupied by persons selected by the Carnegie Institution, the tables are open to whomsoever the director of the laboratory may desire to assign to them.

## RESEARCH ASSISTANTS.

The policy in relation to Research Assistants, as outlined in Year Book No. 2, pp. xlvii–xlviii, was continued, and the persons below named conducted investigations in the branches of science indicated :

| | |
|---|---|
| C. E. Allen, Madison, Wis. Grant No 159. For a study of the homologies of the gametophyte and sporophyte, etc. | $1,000 |
| A. F. Blakeslee, Cambridge, Mass. Grant No. 160. For an investigation of sexuality in the lower fungi. | 1,000 |
| W. W. Coblentz, Cornell University, Ithaca, N. Y. Grant No. 198. For investigating infra-red emission and absorption spectra. | 1,000 |
| A. L. Dean, New Haven, Conn. Grant No. 161. For investigating the proteolytic enzymes of plants | 1,000 |
| L. E. Dickson, University of Chicago, Chicago, Ill. Grant No. 162. For certain mathematical investigations | 1,000 |
| H. W. Doughty, Johns Hopkins University, Baltimore, Md. Grant No 174. For an investigation of camphoric acid, under the direction of Prof. A. A. Noyes | 1,000 |
| C. B. Farrar, Towson, Md. Grant No. 163 For psychological experiments at the Sheppard and Enoch Pratt Hospital | 1,000 |
| William Jones, New York, N. Y. Grant No. 173. For investigating the religion of the central group of Algonkian Indians | 1,000 |
| A. S. King, Bonn, Germany. Grant No. 164. For the production and study of emission spectra at high temperatures | 1,000 |
| P. A. Levene, New York, N. Y. Grant No. 165. For researches along the line of determining points in the constitution of proteids | 1,000 |
| R. S. Lillie, University of Nebraska, Lincoln, Neb. Grant No. 166. For a study of the relation of ions to the various forms of protoplasmic movement | 1,000 |
| G. D. Louderback, San Francisco, Cal. Grant No. 167. For a study of the glaucophane and associated schists | 1,300 |
| F. E. Lutz, Bloomsburg, Pa. Grant No. 142. For study of organic evolution at Station for Experimental Evolution, Cold Spring Harbor, Long Island | 1,000 |
| U. B. Phillips, University of Wisconsin, Madison, Wis. Grant No. 193. For a study of the influence of plantation in political and social history of the South | 300 |
| F. E. Ross, Washington, D. C. Grant No. 168. For astronomical investigation, under Prof. Simon Newcomb | |
| L. S. Rowe, University of Pennsylvania, Philadelphia, Pa. Grant No. 144. For a study of Mexican constitutional system | 1,200 |
| P. E. Sargent, Cambridge, Mass. Grant No. 175. For an investigation in comparative neurology | 1,000 |
| G. W. Scott, Philadelphia, Pa. Grant No. 141. For a study of private claims against foreign nations to which the United States has been a party | 1,200 |
| E. S. Shepherd, Cornell University, Ithaca, N. Y. Grant No. 176. For a systematic study of alloys, with especial reference to brasses and bronzes | 1,000 |
| G. H. Shull, University of Chicago, Chicago, Ill. Grant No. 143. For an investigation in heredity, hybridization, variation, mutation, etc. | 1,000 |
| Mary Roberts Smith, Palo Alto, Cal. Grant No. 194. For studying the history and social conditions of the Chinese immigration in California | 1,000 |
| Nettie M. Stevens, Bryn Mawr College, Bryn Mawr, Pa. Grant No. 177. For an investigation of problems relating to sex determination, etc. | 1,000 |
| J. B. Whitehead, Johns Hopkins University, Baltimore, Md. Grant No. 178. For study of the magnetic effect of electrical displacement | 1,200 |
| E. J. Wilczynski, Berkeley, Cal. Grant No. 135. For an investigation of ruled surfaces, etc | 1,800 |
| Fritz Zerban, Munich, Germany. Grant No. 169. For an investigation of rare earths, under the direction of Prof. C. Baskerville | 1,000 |

## PUBLICATIONS.

The following publications have been issued during the year:

Year Book No. 2, 1903. Octavo, 371 pages.
Report of Committee on Southern and Solar Observatories. Extracted from Year Book No. 2. Octavo, 170 pages.
Desert Botanical Laboratory of Carnegie Institution. Publication No. 6. By F. V. Coville and D. T. MacDougal. Octavo, 58 pages, 29 plates.
New Method of Determining Compressibility. Publication No. 7. By T. W. Richards and W. N. Stull. Octavo, 45 pages, 5 text figures.
Contributions to Stellar Statistics. First paper. On the Position of the Galactic and Other Planes Toward which the Stars Tend to Crowd. Publication No. 10. By Simon Newcomb. Quarto, 30 pages.
Production of Sex in Human Offspring. Publication No. 11. By Simon Newcomb. Octavo, 34 pages.
The Action of Snake Venom upon Cold Blooded Animals Publication No. 12. By Hideyo Noguchi. Octavo, 16 pages.
The Influence of Grenville on Pitt's Foreign Policy, 1787–1798. Publication No. 13. By E. D. Adams. Octavo, 79 pages.
Guide to the Archives of the Government at Washington. Publication No. 14. Octavo, 250 pages.
Fecundation in Plants. Publication No. 15. By D. M. Mottier. Octavo, 187 pp.
Contributions to the Study of the Behavior of the Lower Organisms. Publication No. 16. By H. S. Jennings. Octavo, 256 pages.
Traditions of the Arikara. Publication No. 17. By G. A. Dorsey. Octavo, 202 pages.
Researches on North American Acridiidæ. Publication No. 18. By Albert P. Morse. Octavo, 56 pages, 8 plates.

The following are in press:

Coloration in Polistes. Publication No. 19. By Wilhelmine M. Enteman. Octavo, 88 pages, 6 colored plates.
The coral *Siderastræa radians*. Publication No. 20. By J. E. Duerden. Quarto, 144 pages, 11 plates.
Mythology of the Wichita. Publication No. 21. By G. A. Dorsey. Octavo, 353 pp.
The Waterlilies. Publication No. 22. By H. S. Conard. Quarto, 280 pp., 30 plates.
Bacteria in Relation to Plant Diseases. By Erwin F. Smith. Quarto.
Explorations in Turkestan. By R. Pumpelly, R. W. Pumpelly, W. M. Davis, and Ellsworth Huntington. Quarto.
Collected Mathematical Works of G. W. Hill. It is estimated that these works will make four quarto volumes. Volume I is in type.
Catalogue of Double Stars. By S. W. Burnham. 350 pages in type.

The following are authorized:

Evolution, Racial and Habitudinal, controlled by segregation. By J. T. Gulick.
Chimera—a memoir on the embryology of primitive fishes. By Bashford Dean. Manuscript not received, but plates are prepared.
Bibliographic index of North American fungi. By W. G. Farlow. Will make five octavo volumes. 250 pages in type.
Results of investigations of poison of serpents By Drs. Simon Flexner and Hideyo Noguchi. Manuscript not received.
Heredity of coat characters in guinea pigs and rabbits. By W. E. Castle.
Mutants and hybrids of the Oenotheras. By D T. MacDougal.
Astronomical manuscript. By C. H. F. Peters.
Memoir on fossil cycads. By G. R. Wieland.
Description of the new oxygen apparatus accessory to the calorimeter. By W. O. Atwater.
Rotation of the sun as determined from motion of the calcium flocculi. By G. F. Hale and Philip Fox.

## BIBLIOGRAPHY OF PUBLICATIONS RELATING TO WORK ACCOMPLISHED BY GRANTEES.

In the following list it is sought to include the titles of all publications bearing upon the work done under grants of the Carnegie Institution of Washington. In some cases titles may be included having only an indirect connection with such work.

Abel, John J. The function of the suprarenal glands and the chemical nature of their so-called active principle, pp. 138–165. < Contributions to Medical Research, dedicated to Victor Clarence Vaughan. 1903.

———. Weitere Mittheilungen über das Epinephrin. < Berichte der deutschen chemischen Gesellschaft, Jahrgang XXXVI, Heft 9. June 20, 1903.

———. On epinephrin and its compounds, with especial reference to epinephrin hydrate. < American Journal of Pharmacy. July, 1903.

———. Darstellung und Eigenschaften eines Abbauproductes des Epinephrins. < Berichte der deutschen chemischen Gesellschaft, Jahrgang XXXVII, Heft 2. February 6, 1904.

Atwater, W. O. A respiration calorimeter with appliances for the direct determination of oxygen. < Printed for private circulation; used in connection with an address on the subject at the International Physiological Congress, Brussels. August 30, 1904.

Baskerville, Charles. Thorium, carolinium, berzelium. < Journal American Chemical Society, XXVI, p. 922. August, 1904.

Baskerville, Charles, and Zerban, Fritz. Inactive thorium. < Journal American Chemical Society. To appear.

Boss, Lewis. Catalogue of 627 principal standard stars. < Repriut from a series of articles in the Astronomical Journal. 1903.

Browne, C. E. The cat and the child. < Ped. Sem., XI, pp. 3–29. March, 1904.

———. See G. Stanley Hall and C. E. Browne.

Cannon, William Austin. The spermatogenesis of hybrid peas. < Bulletin of the Torrey Botanical Club. October, 1903.

———. Observations on the germination of *Phoradendron villosum* and *P. californicum* (with 6 figs.). < Bulletin of the Torrey Botanical Club, XXXI, pp. 435–443. 1904.

Carhart, Henry S. The absolute value of the electromotive force of Clark and Weston standard cells. < Transactions of the International Electrical Congress of 1904.

Carlson, A. J. Nervous origin of the heart-beat in Limulus and the nervous nature of coördination or conduction in the heart. < American Journal of Physiology, XII, pp. 67–74. September 1, 1904.

———. The rhythm produced in the resting heart of molluscs on the stimulation of the cardio-accelerator nerves. < American Journal of Physiology, XIII, pp. 55–66. September 1, 1904.

Chrysler, M. A. Anatomical notes on certain strand plants. < Botanical Gazette, XXXVII, pp. 461–464. June, 1904.

Coblentz, W. W. Optical notes. < Physical Review, pp. 89–97. Aug., 1904.

———. Preliminary communication on the infra-red absorption spectra of organic compounds. < Astrophysical Journal, pp. 207–224. October, 1904.

Conard, Henry S. Nymphæa (sub-gen. Brachyceras) in Africa. < Annuaire du Conservatoire et du Jardin Botanique de Genève. May 15, 1903.

Cone, L. H. See M. Gomberg and L. H. Cone.

Coquillett, D. W. Four new species of Culex. < Canadian Entomologist. September, 1903.

———. A new Anopheles with unspotted wings. < Canad. Entom. Nov., 1903.

———. A new Culicid genus related to Corethra. < Canad. Entom. July, 1903.

———. Eucorethra, a genus of Culicidæ. < Canad. Entom. Oct., 1903.

———. Notes on *Culex nigritulus*. < Entomological News. February, 1904.

———. New North American Diptera. < Proceedings Ent. Soc. Washington. July 30, 1904.

COQUILLETT, D. W. Several new Diptera from North America. <Canadian Entomologist. January, 1904.

CREW, HENRY. Normal arc spectra of aluminium and cadmium, being two photographic maps of eight sheets each. <Published privately, through printed circular. November, 1903.

———. On the conditions which govern the appearance of spark lines in arc spectra. <Astrophysical Journal. November, 1904.

CURRIE, R. P. See H. G. Dyar and R. P. Currie.

DAVENPORT, C. B. Color inheritance in mice. <Science, n. s., XIX, pp. 110-114. January 15, 1904.

———. Wonder horses and Mendelism. <Science, n. s., XIX, pp. 151-153. January 22, 1904.

———. Statistical methods, with special reference to biological variation. Second rev. ed. <New York: John Wiley & Sons. Aug. 25, 1904.

DUERDEN, J. E. The antiquity of the Zeanthid Actinians. <Fourth Report of the Michigan Academy of Science. 1904.

———. The morphology of the Madreporaria and septal sequence. <Biological Bulletin, pp. 79-104. July, 1904.

———. Recent results on the morphology and development of coral polyps. <Smithsonian Quarterly. August, 1904.

DYAR, H. G., and CURRIE, R. P. The first stage of *Culex perturbans*. <Proc. Ent. Soc. Washington, VI (in press). October, 1904.

DYAR, HARRISON G. The life history of *Culex cantans* Meg. (corrected later to *C. vittatus* Theop.). <Journal N. Y. Ent. Soc., XII, 36. March, 1904.

———. Life history of *Culex varipalpus* Cog. <Journal N. Y. Ent. Soc., XII, p. 90. June, 1904.

———. The larva of *Culex punctor* Krib., with notes on an allied form (*C. trichurus* n. sp.). <Journal N. Y. Ent. Soc., XII, p. 169. September, 1904.

———. Brief notes on mosquito larvæ. <Journal N. Y. Ent. Soc., XII, p. 172. September, 1904.

———. Notes on the mosquitoes of British Columbia. <Proc. Ent. Soc. Wash., VI, p. 37. January, 1904.

———. The larvæ of the mosquitoes *Megarrhinus rutilus* Cog. and *M. portoricensis* Röd. <Proc. Ent. Soc. Wash., VI, p. 20. January, 1904.

ELVOVE, ELIAS. See J. H. Kastle and E. Elvove.

FARRAR, CLARENCE B. Phenomena of repair in the cerebral cortex. <Histologische und Histopathologische Arbeiten (Nissl). Winter, 1904.

FLEXNER, SIMON, and NOGUCHI, HIDEYO. On the plurality of cytolysins in snake venom. <University of Pennsylvania Med. Bull., XVI, Nos. 5-6, p. 163. July-August, 1903.

———. Upon the production and properties of anti-crotalus venin. <Journal of Medical Research, XI, No. 2, p. 363. May, 1904.

———. On the plurality of cytolysins in normal blood serum. <University of Pennsylvania Med. Bull., XVI, Nos. 5-6, p. 158. July-August, 1903.

FRAZER, J. C. W. See H. N. Morse and J. C. W. Frazer.

GETMAN, F. H. See Harry C. Jones and F. H. Getman.

GOMBERG, M., and CONE, L. H. Über Triphenylmethyl (IX Mittheilung). <Berichte der deutsch. chem. Gesellschaft, XXXVII, pp. 2035-2057. 1904.

HALL, G. STANLEY, and BROWNE, C. E. Children's ideas of fire, heat, frost, and cold. <Pedagogical Seminary, X, pp. 27-85. March, 1903.

HALL, G. STANLEY, and SMITH, THEODATE L. Reactions to light and darkness. <American Journal of Psychology, XIV, pp. 21-83. January, 1903.

———. Marriage and fecundity of college men and women. <Pedagogical Seminary, X, pp. 275-314. September, 1903.

———. Curiosity and interest. <Ped. Sem., X, pp. 315-358. Sept., 1903.

———. Showing off and bashfulness as phases of self-consciousness. <Pedagogical Seminary, X, pp. 159-199. June, 1903.

HAY, OLIVER P. On the finding of skulls of Trionychitæ in the Bridger deposits of Wyoming. <Science, n. s., XIX, p. 254. February 12, 1904.

———. On some fossil turtles belonging to the Marsh collection in Yale University Museum (2 plates and 7 figs). <Am. Jour. of Science, XVIII, pp. 261-276. October, 1904.

HOWARD, L. O. Concerning the geographic distribution of the yellow-fever mosquito. <Public Health Reports, XVIII, No. 46. November 13, 1903.

JONES, HARRY C., and GETMAN, F. H. The existence of alcoholates in solutions of certain electrolytes in alcohol. <American Chemical Journal, XXXII, p. 338. October, 1904.

———. The existence of hydrates in solutions of certain non-electrolytes and the non-existence of hydrates in solutions of organic acids. <American Chemical Journal, XXXII, p. 308. October, 1904.

———. On the nature of concentrated solutions of electrolytes. <American Chemical Journal, XXXI, p. 303. April, 1904.

KASTLE, J. H., and ELVOVE, ELIAS. On the reduction of nitrates of certain plant extracts, and metals and the accelerating effect of certain substances on the progress of the reduction. <American Chemical Journal, XXXI, pp. 606–641. June, 1904.

———. Oxidation and reduction in the animal organism and the toxic action of powerful oxidizing and reducing substances. <American Chemical Journal, XXXI, pp. 195–207. March, 1904.

KING, ARTHUR S. A study of the causes of variability of spark spectra. <Astrophysical Journal. May, 1904.

———. A detailed study of the line spectrum of copper. <Astrophysical Journal. July, 1904.

LEVENE, P. A., and STOOKEY, L. B. On the combined action of proteolytic enzymes. <American Journal of Physiology, XII, p. 1. September 1, 1904.

LEVENE, P. A. The antolysis of animal organs. <American Journal of Physiology, XI, p. 437. July 1, 1904.

———. Hydrolysis of spleen nucleic acid by dilute mineral acid. <American Journal of Physiology, XII, p. 213. October 1, 1904.

———. Darstellung und Analyse einiger Nucliusäuren. <Happe-Zeyler's Zeitschrift für physiologische Chemie, XLIII. November, 1904.

———. (1) The antolysis of animal organs. (2) Hydrolitic cleavage of fresh and self-digested glands. <Am. Jour. Physiology, XII. Nov. 1, 1904.

LEWIS, E. PERCIVAL. The afterglow of metallic vapors in nitrogen—a new band spectrum. <Astrophysical Journal, XX, pp. 49–57. Physikalische Zeitschrift, No. 17, 1904.

———. Notes on the spectra of nitrogen and its oxides. <Astrophysical Journal, XX, pp. 58–62. July, 1904.

LOEB, LEO. On the spontaneous agglutination of blood cells of arthropods. <University of Pennsylvania Medical Bulletin. February, 1904.

———. Über die Koagulation des Blutes einiger Arthropoden. <Hofmeisters Beiträge zur chemischen Physiologie und Pathologie, Band 5. January, 1904.

LOUDERBACK, GEORGE DAVIS. Basin range structure of the Humboldt region. <Bull. Geol. Soc. of America, XV, pp. 289–346 (8 figs.). July, 1904.

MACDOUGAL, D. T. Botanical explorations in the southwest (with 1 pl. and 5 figs.). <Journal N. Y. Bot. Gard., V, pp. 89–91. 1904.

———. Delta and desert vegetation (with 7 figs.). <Bot. Gaz., XXXVIII, pp. 44–63. 1904.

MCLAUGHLIN, A. C. Papers of William Paterson on the Federal Convention, 1787. <Am. Hist. Review. January, 1904.

———. Sketch of Charles Pinckney's plan for a Constitution, 1787. <Am. Hist. Review. July, 1904.

MOORE, J. H. See R. W. Wood and J. H. Moore.

MORSE, A. P. New Acridiidæ from the southeastern States. <Psyche, XI, pp. 7–13. February 20, 1904.

MORSE, H. N., and FRAZER, J. C. W. A new electric furnace and various other electric heating appliances for laboratory use. <Am. Chem. Journal, XXXII. August, 1904.

NOGUCHI, HIDEYO. A comparative study of snake venom and snake sera. <Proc. Pathological Society of Phila., n. s., VI, No. 4, p. 88, Feb., 1903.

———. Effect of snake venom on the blood corpuscles of cold-blooded animals. <Univ. Pennsylvania Med. Bull., XVI, Nos. 5–6, p. 182. July–August, 1903.

NOGUCHI, HIDEYO. On the multiplicity of the serum haem-agglutinins of cold-blooded animals. <Centralblatt für Bakteriologie, Parasitenkunde und Infektions-krankheiten, XXXIV, No. 3, p. 285. 1903.

———. On the heat lability of the complements of cold-blooded animals. <Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten, XXXIV, No. 3, p. 283. 1903.

———. A study of immunization-haemolysins, agglutinins, precipitins, and coagulins in cold-blooded animals. <Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten, XXXIII, No. 5, p. 353. 1903.

———. The interaction of the blood of cold-blooded animals, with reference to hæmolysis, agglutination, and precipitation. <Centralb. für Bakteriologie, Parasitenkunde und Infektionskrankheiten, XXXIII, No. 5, p. 362. 1903.

———. See Simon Flexner and Hideyo Noguchi.

OLIVE, EDGAR W. Mitotic division of the nuclei of the Cyanophyceæ. <Beihefte des Botanisches Centralblatts. November or December, 1904.

OVERTON, JAMES B. Über Parthenogenesis bei Thalictrum purpurascens. Berichte der deutschen botanischen Gesellschaft. Jahrgang, 1904, Band XXII, Heft. 5, pp. 274–281.

PARKHURST, J. A. Nova Geminorum—an early photograph and photographic magnitudes. <Astrophysical Journal, XVII, p. 373. 1903.

———. Photometric magnitudes of comparison. <Popular Astronomy, XI, p. 328. 1903.

———. Stars for Nova Geminorum. <Popular Astronomy, XI, p. 328. 1903.

———. The variable star 6871 V Lyræ. <Astrophys. Jour., XVIII, p. 33. 1903.

———. The variable star 1921 W Aurigæ. <Astrophysical Journal, XVIII, p. 309. 1903.

———. Faint stars near the Trapezium in the Orion nebula. <Astrophysical Journal, XX, p. 136. 1904.

———. Observed magnitudes of 62.1903 Andromedæ. <Astronomical Journal, XXIV, p. 25. 1904.

PEARL, RAYMOND, and BURR, MARY J. A statistical study of conjugation in Paramecium. <Sixth Annual Report of the Michigan Academy of Science. October, 1904.

PERKINS, H. F. Double reproduction in the Medusa hybocodon protifer. <American Naturalist, XXXVIII, Nos. 451–452. July–August, 1904.

PERKINS, I. Marantaceæ of the Philippines, with 3 plates and 31 figures. <I. Perkins Fragmenta Floræ Philippines, II, pp. 67–74. June 30, 1904.

PICKERING, EDWARD C. Carnegie Grant of 1903. <Harvard College Observatory Circular No. 84. August 24, 1904.

———. Nova Geminorum before its discovery. <Harvard College Observatory Circular No. 70. April 3, 1903.

———. Grant from the Carnegie Institution. <Harvard College Observatory Circular No. 69. January 26, 1903.

PUMPELLY, R. Investigation upon ancient sites at Anau, near Askhabad in Russian Turkestan. <In the form of a preliminary letter; by Carnegie Institution of Washington.

RHODES, FREDERICK A. Carbohydrate metabolism; relation of various tissues to destruction of sugar. <American Medicine. December, 1904.

RICHARDS, T. W., and STULL, W. N. New method of determining compressibility, with application to $Br_2$, $I_2$, $CHCl_3$, $CHBr_3$, $CCl_4$, phosphorus, water, and glass. <Carnegie Institution Pub. No. 7. December, 1903.

RICHARDS, T. W. The effects of chemical and cohesive internal pressure. <Proc. Am. Acad., XXXIX, pp. 579–604. June, 1904. Reprinted in full in Zeitschrift für phys. Chemie, XLIX. July, 1904.

SARGENT, PORTER EDWARD. The Torus longitudinalis of the teleost brain: Its ontogeny, phylogeny, morphology, and function. <The Mark Anniversary Volume. May, 1904.

———. The optic reflex apparatus of vertebrates for short-circuit transmission of Motor Reflexes through Reissner's Fibre: Its morphology, ontogeny, phylogeny, and function.—Part I. The fish-like vertebrates. <Bulletin of the Museum of Comparative Zoölogy. July, 1904.

SHEPHERD, E. S. The constitution of the copper-zinc alloys. <Journal of Physical Chemistry, VIII, p. 421. June, 1904.

SMITH, THEODATE L. Types of adolescent affection. <Pedagogical Seminary, XI, pp. 178–203. June, 1904.

———. The psychology of day dreams. <American Journal of Psychology, XV, pp. 465–488. October, 1904.

———. See G. Stanley Hall and Theodate L. Smith.

SPAULDING, EDWARD G. Special physics of segmentation, a synopsis of the parthenogenetic methods. <Biological Bulletin. February, 1904.

———. An establishment of association in hermit crabs, *Eupagurus longicarpus*. <Jour. Comparative Neurology and Psychology. March, 1904.

SPALDING, V. M. Biological relations of certain desert shrubs. I. The creosote bush (*Covillea tridentata*) and its relation to the water supply (with 7 figs.). Botanical Gazette, XXXVIII, pp. 122–138. 1904.

STOOKEY, L. B. See P. A. Levene and L. B. Stookey.

STULL, W. N. See T. W. Richards and W. N. Stull.

WASHINGTON, HENRY S. Analysis of leucite-tephrite from Vesuvius, lava of 1903. <Man. of the Chem. Anal. of Rocks, pp. 168–172. 1904.

WHITEHEAD, JOHN B. Magnetic effect of electric displacement. <Physikalische Zeitschrift, V, No. 11, p. 300. 1904.

WIELAND, G. R. Cycads. <Encyclopedia Americana, vol. V. 1903.

———. Cordaitales. <Encyclopedia Americana, vol. V. 1903.

———. Polar climate in time the major factor in the evolution of plants and animals. <American Journal of Science. December, 1903.

WILSON, EDMUND B. Experimental studies on germinal localization. I. The germ-regions in the egg of Dentalium. <Journal of Experimental Zoölogy, I, No. 1, pp. 1–72, figs. 1–100. May, 1904.

———. Experimental studies on germinal localization. II. Experiments on the cleavage-mosaic in Patella and Dentalium. <Journal of Experimental Zoölogy, I, No. 2, pp. 197–268, figs. 1–118. August, 1904.

WILSON, H. V. Reports on an exploration off the west coasts of Mexico, Central and South America, and off the Galapagos Islands in charge of Alexander Agassiz, etc. XXX. The sponges. <Memoirs of the Museum of Comparative Zoölogy at Harvard University, XXX, No. 1. July, 1904.

WOOD, R. W. Surface color. Prisms and plates for showing dichromatism. <Physical Review. June and August, 1902.

———. Invisibility of transparent objects. <Physical Review. August, 1902.

———. Screens transparent only for ultra-violet light. <Astrophysical Journal. March, 1903.

———. Photographic reversals in spectrum photographs. <Astrophysical Journal. June, 1903.

———. Anomalous dispersion, absorption, and surface color of nitrosodimethyl aniline. <Proc. Am. Acad.; also Phil. Mag. Aug., 1903.

———. The electrical resonance of metal particles for light waves. <Philosophical Magazine. August, 1903.

———. Some new cases of interference and diffraction. <Philosophical Magazine. April, 1904.

———. Quantitative determination of the anomalous dispersion of sodium vapor. <Phil. Mag.; also Proc. Am. Acad. Sci. Sept., 1904.

———. Achromatization of interference bands formed with monochrom. light, and consequent increase in allowable path difference. <Philosophical Magazine. September, 1904.

———. Recent improvements in the diffraction process of color photography. <Nature (London). October, 1904.

———. Apparatus for showing the pressure of sound waves. <Physical Review. November and December, 1904.

WOOD, R. W., and MOORE, J. H. Fluorescence and absorption spectra of sodium vapor. <Astrophysical Journal; also Physikalische Zeitschrift. September, 1903.

ZERBAN, FRITZ. See Charles Baskerville and Fritz Zerban.

# ACCOMPANYING PAPERS.

## LIST OF ACCOMPANYING PAPERS

# STUDY OF CONDITIONS FOR SOLAR RESEARCH AT MOUNT WILSON, CALIFORNIA.

BY GEORGE E. HALE.

In 1902, Dr. S. P. Langley addressed a communication to the Carnegie Institution recommending the establishment of an observatory at a very high altitude for the special purpose of measuring the solar radiation. In this communication Dr. Langley offered reasons for his belief that the solar radiation may undergo changes of intensity corresponding with those great changes of solar activity which are so strikingly illustrated in the sun-spot cycle. This communication was referred to an advisory committee appointed by the Carnegie Institution to report on various astronomical projects which had been submitted. The committee consisted of Prof. E. C. Pickering, chairman; Prof. Lewis Boss, Dr. S. P. Langley, Prof. Simon Newcomb, and the writer. In its report to the Carnegie Institution, the committee expressed its approval of Dr. Langley's proposal and recommended, in case the Institution felt inclined to pursue the matter further, that a special committee be appointed to make a detailed report on the requirements of a complete solar observatory. It was also recommended that a project for an observatory in the southern hemisphere be investigated and reported upon by the same committee.

As a result of this recommendation, a committee, consisting of Prof. Lewis Boss, chairman; Prof. W. W. Campbell, and the writer, was appointed in December, 1902, to report upon the proposed southern and solar observatories. The report of this committee may be found in Year Book No. 2 of the Carnegie Institution. This report also includes a detailed account by Prof. W. J. Hussey of his telescopic tests of atmospheric conditions at sites in Southern California and Arizona, where he had been sent by the committee. Professor Hussey strongly recommended, as the result of his tests, that Mount Wilson, near Pasadena, California, be chosen as the site of the proposed solar observatory, in case the Carnegie Institution decided to establish it.

My first visit to Mount Wilson was made in company with Professor Campbell in June, 1903. Professor Hussey had practically completed his tests and desired that we should see for ourselves the conditions he had found. Previous observations of the sun at Pike's Peak, Mount Etna, and Mount Hamilton had in no wise prepared me

for my experience on Mount Wilson. On certain occasions, it is true, I had seen the solar image sharply defined on Mount Etna in the very early morning hours. On Mount Hamilton, also, the solar image is sometimes good; but the testimony of those who have observed the sun there was decidedly unfavorable. It was therefore with intense satisfaction that on each of the four days of my stay on Mount Wilson I found the definition of the solar image almost perfect, to be rated at from 8 to 9 on a scale of 10.

This visit was necessarily a hurried one, and it was evident that before Mount Wilson could be determined upon as the best available site for an observatory, observations extending over a long period of time would be necessary. As circumstances required that my family should spend the winter of 1903-1904 in Southern California, I decided to take this opportunity to make a more complete test of atmospheric conditions on Mount Wilson. Before arrangements had been made for living upon the mountain, I made frequent trips from Pasadena to Mount Wilson during the months of December, January, and February, observing the sun on each occasion with a telescope of 3¼ inches aperture, and noting the prevailing weather conditions. The extraordinary absence of wind, which had seemed so characteristic a feature of the mountain during Professor Hussey's visit, could not be said to continue throughout the winter months. High gales sometimes occur at this season, and the average wind velocity is greater than during the summer. Nevertheless, the wind during the day was usually very light, and on many occasions the quiet days of the previous June seemed to be almost exactly duplicated, except that the temperature was lower. For weeks together not a cloud would be seen in the sky, and the summer serenity was in some measure retained until well into January. Later it was broken by storms, but these practically ended with April.

As the solar definition proved to be surprisingly good for this season of the year, I was soon convinced that Mount Wilson offered exceptional opportunities for both solar and stellar work and that a systematic test of conditions should be inaugurated at the earliest possible moment. Accordingly, I commenced on March 1 to render habitable an old log cabin on the mountain that had been in a state of partial ruin for many years. This cabin, known locally as the "Casino," became our headquarters, where we have lived throughout our work on Mount Wilson. Tests of the solar definition were made as often as possible with the telescope already mentioned, and on April 15 several meteorological instruments provided by the

Carnegie Institution were installed. Since that time, with only such interruptions as have been made necessary by the enforced absence of the observers, the instruments have been read at stated hours by Mr. Ferdinand Ellerman or Mr. W. S. Adams, who have also made regular tests of the seeing with the telescope mentioned above.

Through important financial assistance rendered by Mr. Arthur Orr, of Evanston, Illinois, and Mr. John D. Hooker, of Los Angeles, and the exceptional facilities kindly granted by the Atchison, Topeka and Santa Fe Railway Company, through President Ripley, it became possible to bring from the Yerkes Observatory the small coelostat which had previously been sent to the eclipses of 1900 (North Carolina) and 1902 (Sumatra). It had been my purpose to bring out the Snow telescope, but lack of sufficient funds prevented me from doing so. The smaller coelostat was accordingly erected on the mountain, where it yielded excellent photographs of the sun, amply sufficient to give objective evidence of the high quality of the observational conditions.

During my first visit to Mount Wilson the only unfavorable feature was the presence of fine dust in the air, which was conspicuous not only in the valley below, but also seemed to extend to a considerable altitude above the mountain. This was by no means sufficient to affect greatly the transparency of the sky, except very near the horizon. Nevertheless, the Milky Way did not stand out with the degree of contrast which one expects to see in a very transparent atmosphere. On my return trip to Chicago through the San Gabriel Valley the dust seemed so conspicuous that I feared it might prove an important objection to Mount Wilson as a site for an observatory. In most classes of solar observation dust does not play a very important part, and the great steadiness of the image would far outweigh any objections which might result from this cause. But in other classes of work which were contemplated for the proposed observatory, this dust, if persistent, would inevitably prove a serious obstacle. For example, in determinations of the value of the solar constant and in the photography of faint nebulæ, the absorption and scattering of light produced by dust in the atmosphere may interfere greatly with the work. It accordingly seemed that special attention should be given to the question of dust in the atmosphere above Mount Wilson. It has fortunately turned out, as will be shown later, that the presence of any appreciable amount of dust in the air above the mountain is so exceptional a phenomenon as to constitute no important objection to Mount Wilson as an observatory site.

After a brief statement regarding the conditions found at Mount Wilson had been presented to the Executive Committee of the Carnegie Institution, in April, 1904, they decided to make a grant of a sum sufficient to provide for the erection and use of the Snow telescope on the mountain. The Yerkes Observatory loaned the telescope and the University of Chicago provided the salaries of some of the observers. The work accomplished on the mountain since this grant was made has been sufficient to serve as a reliable basis for estimates on the cost of a large solar observatory, besides giving valuable experience regarding the necessary methods and cost of construction under the unusual conditions existing at the summit of a mountain nearly 6,000 feet in height. In view of their bearing on the question of a solar observatory, I have accordingly included in my report some remarks on the principal obstacles encountered and overcome in the construction of buildings and the transportation of instruments and materials.

### REQUIREMENTS OF A SITE FOR A SOLAR OBSERVATORY.

It is desirable to recapitulate here the purposes and plans for a solar observatory which were given at some length in Year Book No. 2 of the Carnegie Institution. At the outset, it should be stated that the term "solar observatory" is used here in a broad sense, since it is not intended to exclude from the program certain investigations of stars which are of fundamental importance in any general study of the problem of stellar evolution. For the sun is a star, comparable in almost every respect with many other stars in the heavens, and rendering possible, through an intimate knowledge of its own phenomena, the solution of some of the most puzzling questions in the general problem of stellar evolution. Conversely, however, the stars are suns, and if we would know the past and future conditions of the sun, we must examine into the physical condition of stars which represent earlier and later stages of development. It will be seen that there is ample ground for the inclusion in the equipment of a solar observatory of certain instruments especially designed for the study of stellar problems.

The plan of work proposed for the observatory, as outlined in Year Book No. 2, includes the following classes of observations:

(1) Frequent measurements of the heat radiation of the sun, to determine whether there may be changes during the sun-spot cycle in the amount of heat received from the sun by the earth and in the relative radiation of the various portions of the solar surface.

(2) Studies of various solar phenomena, particularly through the use of powerful spectroscopes and spectroheliographs.

(3) Photographic and spectroscopic investigations of the stars and nebulæ with a very powerful reflecting telescope, for the principal purpose of throwing light on the problem of stellar evolution.

The present opportunity for important advances in these three departments of research is very unusual. Since the publication of Year Book No. 2, Dr. Langley has offered reasons to believe that an actual change in the amount of heat emitted by the sun occurred in March, 1903. It is hardly necessary to say that a change in the intensity of the sun's heat, if actually established, might have a most important bearing upon many questions relating to the earth, and, at the same time, be of capital interest in its relationship to the problem of the solar constitution. Through the force of circumstances, Dr. Langley's observations have been made under the very unfavorable conditions which obtain at Washington. If they could be continued at a considerable altitude, at a point above the denser and more fluctuating region of the earth's atmosphere, the question as to what changes actually occur in the solar radiation could doubtless be answered in a thoroughly satisfactory manner.

In the study of the phenomena of the sun's surface and atmosphere we again enter a remarkably fruitful field of research. Within the past few years the instruments available for work in this field have been greatly developed, and now only await application on a large scale in order to secure a great number of new results which have hitherto been entirely out of reach. But even if the means were available for supplying the necessary instruments to existing observatories, they could not be successfully employed without atmospheric conditions much superior to those at present available. In work of this nature, success depends upon the perfect definition of the solar image and the absence of those disturbances from which the atmosphere at existing observatories is almost never free. For this work, therefore, an elevated station in a region of great atmospheric calm is absolutely essential. Furthermore, the site must be free from the disturbing factors which frequently prevent good observations from being obtained on mountain summits.

In the third class of investigations required to complete the program of a properly equipped solar observatory, similar possibilities of advance exist. Within the past few years the remarkable advantages of the reflecting telescope have been demonstrated. It now only remains to construct a large and powerful instrument

of the type shown by these experiments to promise success. With such an instrument, immense new fields of investigation of the highest importance in their bearing on the problem of stellar evolution could be immediately occupied. Here again, however, the unfavorable atmospheric conditions at almost all existing observatories would render the construction of a large telescope almost useless. To be successful, such an instrument must be erected at a site where the night-seeing is nearly perfect, the sky clear and transparent, and the average wind velocity very low. Under such conditions, a properly constructed telescope of large aperture would undoubtedly yield results greatly surpassing those hitherto obtained.

These considerations are sufficient to define the general character of a site suitable for a well-equipped solar observatory. There are other points, however, which must be taken into account. A solar observatory provided with an outfit of instruments, and then left to do its work without the possibility of improvement or change, could never attain the best results. On the contrary, it must have the means of producing new types of instruments and modifying old ones, as the development of the work may suggest. In other words, a shop completely equipped with all appliances necessary for the most refined construction of both the mechanical and optical parts of instruments, should form an integral part of a solar observatory. A shop of this kind can not be conducted without great difficulty and expense if far removed from large cities and other sources of supply. This is only one of many reasons which would render it desirable to select an observatory site within easy reach of the facilities afforded by a large city.

In his recommendation for the establishment of an observatory for the purpose of determining whether the heat radiation of the sun undergoes change, Dr. Langley pointed out the desirability of making the observations at a height of some 20,000 feet above sea-level. Apart from the excessive difficulty and expense of conducting an observatory at such an elevation, which are best appreciated by those who have worked at great altitudes, the inaccessibility of high mountain peaks would stand in the way of such an undertaking. But it nevertheless might have been carried out, at a somewhat lower altitude, if the recent development of Dr. Langley's work at Washington had not indicated that the great mass of observations could undoubtedly be made to good advantage at a much lower station. The increasing perfection of the observational method has, indeed, permitted fairly good results to be obtained under the very unfavorable

conditions which exist at Washington. Nevertheless, it by no means follows that Dr. Langley's purpose could be accomplished at such a point. The humidity of our atmosphere is a most serious obstacle in this particular work, since the solar heat is very subject to absorption by water vapor. It is therefore desirable to establish the instruments at least a mile above the dense and disturbed layers of the atmosphere which lie near the sea-level. Certain problems connected with the investigation may render it desirable to make some of the observations at a higher altitude, reaching from 12,000 to 15,000 feet. We conclude, therefore, that the principal work should be done at a station having an elevation of 5,000 to 6,000 feet, in a dry climate, where the weather is continuously clear over long periods of time. The work at higher altitudes, if needed at all, could in all probability be completed in two or three summers by expeditions equipped with a portable outfit erected at an altitude of from 12,000 to 15,000 feet. It would thus be convenient to have the principal station at a lower altitude, not far removed from accessible mountains of this considerable elevation. It would be inadvisable, for reasons which it is hardly necessary to specify, to establish the principal station at an altitude much greater than 6,000 feet.

### POSITION AND NATURAL RESOURCES OF MOUNT WILSON.

From a meteorological standpoint, the State of California may naturally be divided into three parts. In the northern region the rainfall is very considerable, much cloudiness prevails, and in almost all respects the conditions are very unfavorable for astronomical work. The central region, which may be considered to extend as far south as Point Concepcion, is favored with much better weather conditions, best exemplified at the Lick Observatory, on Mount Hamilton, where a high average of night-seeing is maintained during a large part of the year. Except for the frequent winds at night, which interfere with some classes of work, Mount Hamilton might be regarded as an almost ideal observatory site, at least for night observations. For solar work it may not be superior to certain stations in the eastern part of the United States, because of the excessive radiation from the heated slopes of the mountain, which is almost devoid of trees near the summit.

In the southern part of California the climatic conditions are decidedly different from those which prevail in the two other sections of the State. The much lighter rainfall is naturally associated with fewer clouds, a remarkably steady barometer, and very light winds. During a part of the year the fog rolls in from the

ocean and covers much of the San Gabriel Valley during the night. But these fog-clouds rarely attain elevations exceeding 3,000 feet, except when storm conditions prevail during the winter months. The mountains of the Sierra Madre range rise high above the fog, and during a great proportion of the year they enjoy practically continuous sunshine. During the summer months the sea breeze blows for a large part of the day, but it attains only a low velocity, which decreases in passing from the valley to the mountain tops and in going inward from the coast.

Mount Wilson is one of many mountains that form the southern boundary of the Sierra Madre range. Standing at a distance of 30 miles from the ocean, it rises abruptly from the valley floor, flanked only by a few spurs of lesser elevation, of which Mount Harvard is the highest. Except for a narrow saddle, Mount Wilson is separated from Mount Harvard by a deep canyon, the walls of which are very precipitous. Farther to the west, beyond the saddle leading to Mount Harvard, the ridge of Mount Wilson forms the upper extremity of Eaton Canyon, which leads directly to the San Gabriel Valley. East and north of Mount Wilson lies the deep canyon through which flows the west fork of the San Gabriel River, and beyond this rises a constant succession of mountains, most of them higher than Mount Wilson, which extend in a broken mass to the Mojave Desert. The Sierra Madre range forms the northern boundary of the San Gabriel Valley, which is further protected toward the east from the desert by the high peaks of the San Bernardino range. Through the Cajon Pass, where the Atchison, Topeka and Santa Fe Railroad enters the valley, winds from the desert frequently blow, bringing vast quantities of dust, which sometimes diffuses through the lower air over the entire valley. This dust but rarely reaches an elevation as great as that of Mount Wilson, though I have seen a few wind-storms that carried the dust of the desert directly over the Sierra Madre range and into the valley below.

For the most part, the readily accessible mountains on the southern boundary of the Sierra Madre range have few trees near the summit, and enjoy but small supplies of water. Mount Wilson is remarkable in having a fine growth of trees covering its summit, and in possessing within easy reach of its highest point several large springs of water, which afford a good supply even during very dry seasons.

In a dry country the question of a pure and permanent supply of water is of paramount importance. It is therefore desirable to give

more definite information of the springs near the summit of Mount Wilson. Some of these are located at Strain's Camp, where, for many years, they have supplied the necessities of summer visitors, who frequently occupy tents here for considerable periods of time. Two wells have been dug at Strain's Camp, and these are regarded as excellent sources of pure water.

In accordance with the terms of the lease of the property at present occupied as an observatory site on Mount Wilson, the water rights on the mountain are to be equally divided between the owners of the property and the occupants of the observatory site. It seems probable that the wells at Strain's Camp, if properly developed, would supply the purposes of a large observatory. If not, more water could easily be developed on the mountain; it may appear desirable to obtain water from a stream in one of the neighboring canyons, about 1,000 feet below. The expense of pumping to this height would not be great, and the stream can be relied upon as a never-failing source of water. A water-tunnel on the south face of the mountain has been reserved by the owners of the property for the purpose of supplying Martin's Camp, and is not included in the equal division of the remaining water rights. A method of securing more water, which could undoubtedly be employed with advantage, would be through the use of large storage tanks, in which water could be collected during the rainy season, either by pumping from the overflowing wells or by catching the rain as it falls on roofs or other large surfaces provided for the purpose.

## TRANSPORTATION AND CONSTRUCTION.

Much granite is available on Mount Wilson for the purpose of construction, but in the portion of the mountain selected for the observatory site it is not so easily obtained as might be wished. This is due to the fact that much of the granite is decomposed, and consequently too soft for building purposes. The hard and the decomposed granites occur together, so that if a quarry is started at a point where plenty of hard granite seems to be present, it sometimes happens that the supply is soon exhausted, leaving only decomposed granite below. Men experienced in matters of this kind have been quite unable to judge whether selected spots could be relied upon to furnish a good supply of hard granite. This fact greatly increases the expense of constructing stone piers, since quarries may have to be abandoned after having been opened at considerable cost. However, some abundant sources of excel-

lent stone can be rendered easily accessible by the extension of roads constructed for work now in progress.

Numerous fallen trees on Mount Wilson, which are not yet greatly decayed, will furnish an abundant supply of fire-wood for many years to come. They can not be depended upon, however, to yield any wood for building purposes, and as the living trees may not be destroyed, all lumber must be taken to the summit of the mountain from Pasadena. This raises the question of transportation over the mountain trail—a matter of vital importance in constructing an observatory. The "Toll Road" or "New Trail," which extends from the summit of the mountain to the foot of Eaton Canyon, is well adapted for all ordinary packing with animals, though it is much too narrow to permit wagons to pass over it. At present, all except the heaviest articles are taken to the summit of the mountain by means of burros and pack-mules, each of which can carry a load ranging from 80 to 200 pounds. It is evident that transportation of building materials by this means must be very slow and expensive, since the trail is 9 miles in length to the foot of Eaton Canyon, 6½ miles distant by road from Pasadena. But, as compared with most mountains, Mount Wilson is unusually accessible from cities, Pasadena being so close at hand, and Los Angeles, with its large sources of supply, being only 9 miles farther away.

For transporting heavy castings and other similar articles, we have found it necessary to construct a special four-wheel carriage, 2 feet in width. On this loads of a thousand pounds have been taken to the summit without difficulty. By widening the trail to 6 feet, the heaviest castings required for a solar observatory probably could be transported.

### WEATHER.

So far as cloudiness is concerned, the records of the Weather Bureau at Los Angeles are of comparatively little value for our present purposes. The fog rolls in from the ocean night after night, and sometimes hangs over Los Angeles throughout the day during the winter season. But Mount Wilson reaches far above this layer of clouds, and thus frequently enjoys sunshine when the valley below is completely covered. Our daily percentage record of cloudiness, beginning on April 18, 1904, may be found in the following table. A dash signifies that no observation was made.

There were many days which were cloudy at the time of observation, but nevertheless suitable at other hours for solar work. Adding these to the record, it may be said that the actual number of days

CLOUDINESS.

| Day of month. | April. | | May. | | June. | | July. | | August. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. |
| 1 | ........ | ........ | 100 | 20 | 75 | 75 | 0 | 0 | 0 | 0 |
| 2 | ........ | ........ | 0 | 5 | 5 | 0 | 0 | 0 | 0 | 0 |
| 3 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 90 | 10 |
| 4 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 5 | 25 |
| 5 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 6 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 30 | 100 |
| 7 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | — | 40 | 60 |
| 8 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 10 | 50 | 40 |
| 9 | ........ | ........ | 79 | 85 | 0 | 0 | 0 | — | 0 | 0 |
| 10 | ........ | ........ | 5 | 5 | 0 | 0 | — | — | 0 | 0 |
| 11 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 12 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 30 | 50 |
| 13 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 20 | 20 |
| 14 | ........ | ........ | 0 | 0 | 0 | 0 | 0 | 0 | 80 | 80 |
| 15 | ........ | ........ | 0 | 0 | 0 | 25 | 0 | 0 | 0 | 0 |
| 16 | ........ | ........ | 0 | 0 | 5 | 0 | 0 | — | 0 | 5 |
| 17 | ........ | ........ | 0 | 0 | 0 | 0 | — | — | 0 | 0 |
| 18 | ........ | ........ | 0 | 0 | 0 | 10 | — | 0 | 10 | 5 |
| 19 | 100 | 100 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 20 | 50 | 5 | 0 | 75 | 0 | 0 | 5 | 5 | 0 | 0 |
| 21 | 0 | 0 | 25 | 5 | 0 | 0 | — | 50 | 0 | 5 |
| 22 | 70 | 100 | 0 | 50 | 0 | 0 | 45 | 5 | 75 | 80 |
| 23 | 0 | 0 | 5 | 5 | 0 | 0 | 35 | 5 | 20 | 30 |
| 24 | 0 | 5 | 0 | 0 | 0 | 0 | 5 | 10 | 60 | 5 |
| 25 | 75 | 0 | 100 | 100 | 0 | 0 | 35 | 15 | 5 | 5 |
| 26 | 100 | 100 | 100 | 0 | 0 | 0 | 20 | 15 | 5 | 5 |
| 27 | 0 | 100 | 0 | 0 | 0 | 0 | 12 | 7 | 0 | 5 |
| 28 | 80 | 100 | 0 | 0 | 0 | 0 | 15 | 5 | 0 | 5 |
| 29 | 0 | 0 | 10 | 75 | 0 | 0 | 10 | 5 | 0 | 5 |
| 30 | 0 | — | 0 | 0 | 0 | 0 | 5 | — | 5 | 80 |
| 31 | ........ | ........ | 0 | 0 | ........ | ........ | — | — | 80 | 5 |

on which observations could be made amount to 132 out of 135. The long periods of perfectly clear weather, permitting observations of the sun to be made without interruption from day to day, should prove of the greatest importance in the study of many solar problems which require daily observations for their solution. From the records so far obtained, it seems probable that observations of the sun could be made at Mount Wilson on more than 300 days in a year. In Los Angeles, during the past twenty-three years, the average number of "clear" days in the year is 317.

The cloudiness in July and August was due almost entirely to thunderstorms over the desert to the north and east. The clouds rarely reached our zenith and almost never interfered with the regu-r solar observations (cf. table of Seeing, pp. 170, 171).

HUMIDITY.

The question of humidity is of special importance in connection with the measurement of the solar constant, since water-vapor in the atmosphere absorbs very strongly the solar heat. The results obtained with a standard sling psychrometer, Weather Bureau pattern, are given in the following table.

RELATIVE HUMIDITY AT MOUNT WILSON.

| Day of month. | April. | | May. | | June. | | July. | | August. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. | 8 a. m. | 6 p. m. |
| 1 | ........ | ........ | 100 | 80 | 40 | 36 | 34 | 24 | 22 | 27 |
| 2 | ........ | ........ | 57 | 80 | 39 | 39 | 23 | 38 | 22 | 33 |
| 3 | ........ | ........ | 34 | 66 | 39 | 41 | 30 | 32 | 29 | 30 |
| 4 | ........ | ........ | 46 | 49 | 24 | 25 | 38 | 56 | 32 | 40 |
| 5 | ........ | ........ | 38 | 38 | 20 | 24 | 34 | 42 | 40 | 43 |
| 6 | ........ | ........ | 29 | 40 | 27 | 42 | 45 | 43 | 34 | 43 |
| 7 | ........ | ........ | 36 | 31 | 29 | 41 | 49 | — | — | 76 |
| 8 | ........ | ........ | 18 | 22 | 23 | 29 | 33 | 18 | 64 | 58 |
| 9 | ........ | ........ | 15 | 24 | 20 | 34 | 42 | — | 41 | 46 |
| 10 | ........ | ........ | 22 | 41 | 21 | 21 | — | — | 28 | 35 |
| 11 | ........ | ........ | 36 | 38 | 19 | 17 | 57 | 24 | 30 | 33 |
| 12 | ........ | ........ | 36 | 54 | — | — | 29 | 40 | 53 | 65 |
| 13 | ........ | ........ | 41 | 40 | — | 43 | 46 | 22 | 61 | 64 |
| 14 | ........ | ........ | 32 | 64 | 52 | 42 | 25 | 25 | 67 | 72 |
| 15 | ........ | ........ | 23 | 29 | 32 | 30 | 30 | 44 | 54 | 45 |
| 16 | ........ | ........ | 27 | 19 | 34 | 30 | 50 | — | 37 | 36 |
| 17 | ........ | ........ | 34 | 38 | 33 | 24 | — | — | 40 | 30 |
| 18 | — | 85 | 25 | 66 | 32 | 41 | — | 16 | 34 | 45 |
| 19 | 100 | 100 | 46 | 64 | 31 | 42 | 22 | 24 | 32 | 46 |
| 20 | 96 | 73 | 41 | 100 | 56 | 72 | 21 | 19 | 23 | 40 |
| 21 | 60 | 79 | 42 | 45 | 45 | 53 | — | 42 | 37 | 33 |
| 22 | 92 | 98 | 28 | 43 | 40 | 43 | 24 | 34 | 58 | 85 |
| 23 | 63 | 65 | 37 | 32 | 44 | 54 | 35 | 35 | 79 | 79 |
| 24 | 60 | 50 | 38 | 50 | 42 | 32 | 31 | 26 | 77 | 56 |
| 25 | 29 | 68 | 100 | 100 | 30 | 27 | 33 | 50 | 69 | 58 |
| 26 | 100 | 100 | 100 | 48 | 28 | 25 | 38 | 27 | 67 | 37 |
| 27 | 100 | 100 | 37 | 38 | 27 | 36 | 31 | 50 | 40 | 43 |
| 28 | 100 | 100 | 38 | 33 | 42 | 35 | 30 | 32 | 43 | 33 |
| 29 | 46 | 36 | 23 | 15 | 35 | 30 | 50 | 46 | 21 | 29 |
| 30 | 58 | 71 | 14 | 47 | 30 | 37 | 38 | — | 27 | 14 |
| 31 | ........ | ........ | 30 | 35 | ........ | ........ | — | — | 14 | 31 |
| Means | 77 | | 43 | | 34 | | 33 | | 43 | |

The marked dryness of the atmosphere on Mount Wilson during the summer months may be best appreciated by comparing these results with those obtained by the Weather Bureau at Washington during the corresponding period.

RELATIVE HUMIDITY AT WASHINGTON.

| Month. | Mean. | Maximum.* | Minimum.* |
|---|---|---|---|
| April | 63.0 | 100 | 30 |
| May | 65.6 | 97 | 41 |
| June | 77.8 | 98 | 54 |
| July | 60.6 | 99 | 51 |
| August | 78.0 | 95 | 59 |

* Mean maximum and mean minimum humidity not determined.

### TEMPERATURE.

From March 25 to April 15 the temperature was recorded on a self-registering thermometer. After April 15 this record was supplemented by observations of maximum and minimum thermometers. The results are given (in degrees Fahrenheit) in the following table. As bearing upon certain classes of night observations, the range of temperature between 8 p. m. and 4 a. m. is also included.

TEMPERATURE.

| Day of month. | April. | | | May. | | | June. | | | July. | | | August. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Maximum. | Minimum. | Range 8 p. m. to 4 a. m. | Maximum. | Minimum. | Range 8 p. m. to 4 a. m. | Maximum. | Minimum. | Range 8 p. m. to 4 a. m. | Maximum. | Minimum. | Range 8 p. m. to 4 a. m. | Maximum. | Minimum. | Range 8 p. m. to 4 a. m. |
| 1 | ........ | ........ | ........ | 49 | 32 | 8 | 76 | 46 | 4 | 93 | 57 | 5 | 83 | 57 | 3 |
| 2 | ........ | ........ | ........ | 56 | 27 | 4 | 82 | 53 | 1 | 81 | 58 | 5 | 90 | 63 | 5 |
| 3 | ........ | ........ | ........ | 64 | 39 | 4 | 89 | 55 | 2 | 79 | 56 | 8 | 91 | 66 | 3 |
| 4 | ........ | ........ | ........ | 70 | 42 | 5 | 80 | 56 | 7 | 80 | 56 | 3 | 89 | 66 | 5 |
| 5 | ........ | ........ | ........ | 79 | 45 | 7 | 81 | 59 | 5 | 77 | 54 | 6 | 91 | 66 | 2 |
| 6 | ........ | ........ | ........ | 80 | 55 | 2 | 78 | 57 | 6 | 75 | 55 | 6 | 92 | 68 | 2 |
| 7 | ........ | ........ | ........ | 84 | 51 | 8 | 75 | 48 | 9 | — | — | 5 | — | — | 6 |
| 8 | ........ | ........ | ........ | 84 | 55 | 7 | 79 | 52 | 3 | 76 | 52 | 6 | 83 | 64 | 3 |
| 9 | ........ | ........ | ........ | 85 | 57 | 5 | 83 | 57 | 4 | — | — | 2 | 87 | 60 | 6 |
| 10 | ........ | ........ | ........ | 86 | 57 | 5 | 82 | 61 | 5 | — | — | 2 | 86 | 64 | 4 |
| 11 | ........ | ........ | ........ | 79 | 57 | 2 | 87 | 60 | 4 | 82 | 57 | 5 | 87 | 63 | 4 |
| 12 | ........ | ........ | ........ | 85 | 54 | 2 | — | — | 5 | 86 | 56 | 4 | 88 | 62 | 5 |
| 13 | ........ | ........ | ........ | 75 | 56 | 4 | — | — | 5 | 89 | 59 | 3 | 85 | 64 | 3 |
| 14 | ........ | ........ | ........ | 79 | 50 | 6 | 77 | 57 | 6 | 83 | 63 | 7 | 83 | 60 | 3 |
| 15 | ........ | ........ | ........ | 85 | 53 | 4 | 80 | 54 | 4 | 75 | 57 | 4 | 86 | 60 | 2 |
| 16 | ........ | ........ | ........ | 82 | 57 | 4 | 79 | 53 | 5 | 72 | 50 | 4 | 87 | 60 | 5 |
| 17 | ........ | ........ | ........ | 79 | 56 | 5 | 81 | 56 | 5 | 84 | 63 | 6 | 87 | 62 | 5 |
| 18 | 53 | 37 | 6 | 75 | 49 | 4 | 83 | 55 | 8 | 93 | 61 | 1 | 88 | 63 | 4 |
| 19 | 42 | 35 | 3 | 56 | 36 | 8 | 79 | 59 | 5 | 95 | 66 | 3 | 93 | 64 | 5 |
| 20 | 41 | 23 | 6 | 74 | 45 | 1 | 73 | 51 | 7 | 94 | 69 | 1 | 90 | 65 | 5 |
| 21 | 49 | 34 | 1 | 78 | 50 | 5 | 86 | 50 | 1 | — | — | 2 | 86 | 59 | 8 |
| 22 | 46 | 32 | 5 | 81 | 51 | 4 | 83 | 55 | 2 | 82 | 62 | 4 | 77 | 60 | 2 |
| 23 | 49 | 25 | 9 | 78 | 56 | 3 | 78 | 55 | 6 | 89 | 62 | 2 | 78 | 56 | 2 |
| 24 | 63 | 36 | 3 | 69 | 54 | 4 | 78 | 53 | 6 | 91 | 66 | 3 | 81 | 56 | 0 |
| 25 | 64 | 40 | 4 | 52 | 38 | 10 | 87 | 56 | 3 | 88 | 68 | 4 | 85 | 61 | 2 |
| 26 | 43 | 29 | 8 | 53 | 35 | 3 | 88 | 62 | 1 | 82 | 66 | 3 | 89 | 62 | 3 |
| 27 | 43 | 23 | 4 | 69 | 41 | 0 | 85 | 63 | 5 | 88 | 62 | 2 | 87 | 64 | 2 |
| 28 | 44 | 31 | 0 | 75 | 49 | 5 | 79 | 57 | 4 | 83 | 68 | 5 | 87 | 62 | 2 |
| 29 | 63 | 36 | 2 | 71 | 50 | 5 | 79 | 55 | 3 | 83 | 62 | 7 | 87 | 62 | 2 |
| 30 | 65 | 41 | 5 | 70 | 46 | 6 | 78 | 53 | 5 | — | — | 3 | 81 | 59 | 4 |
| 31 | — | — | — | 77 | 45 | 4 | — | — | — | — | — | 4 | 82 | 59 | 3 |
| Means | 51.2 | 32.7 | 4.3 | 73.5 | 48.0 | 4.7 | 80.9 | 55.3 | 4.5 | 84.0 | 60.2 | 4.0 | 86.2 | 61.9 | 3.5 |
| Daily range | 18.5 | | ........ | 25.5 | | ........ | 25.6 | | ........ | 23.8 | | ........ | 24.3 | | ........ |

### ATMOSPHERIC PRESSURE.

No complete barometric record has been kept, since this did not seem of special importance in connection with the work. Nevertheless, an aneroid barometer has been read twice daily since July 13. The maximum and minimum readings recorded up to September 1 differed by only 0.22 inch.

### WIND MOVEMENT.

With such uniformity of atmospheric pressure, it might naturally be anticipated that the wind movement would be low. The results of anemometer readings (in miles), made with an instrument of the standard Weather Bureau pattern, are shown in the following table. The "day" results give the total movement from 8 a. m. to 6 p. m.; the "night" results give the total movement from 6 p. m. to 8 a. m.

| Day of month. | April. | | May. | | June. | | July. | | August. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Day. | Night. | Day. | Night. | Day. | Night. | Day. | Night. | Day. | Night. |
| 1 | ...... | ...... | 99 | 75 | 120 | 175 | 55 | 145 | 43 | 95 |
| 2 | ...... | ...... | 49 | 165 | 71 | 141 | 71 | 102 | 50 | 72 |
| 3 | ...... | ...... | 52 | 118 | 44 | 185 | 47 | 71 | 45 | 61 |
| 4 | ...... | ...... | 29 | 88 | 44 | 114 | 34 | 41 | 46 | 48 |
| 5 | ...... | ...... | 43 | 91 | 61 | 47 | 30 | 54 | 60 | 41 |
| 6 | ...... | ...... | 30 | 62 | 78 | 108 | 43 | 78 | 43 | 49 |
| 7 | ...... | ...... | 34 | 128 | 59 | 155 | 40 | 44 | 47 | 80 |
| 8 | ...... | ...... | 43 | 106 | 55 | 146 | 49 | 85 | 33 | 63 |
| 9 | ...... | ...... | 38 | 44 | 50 | 66 | 44 | 83 | 43 | 60 |
| 10 | ...... | ...... | 44 | 133 | 52 | 86 | 43 | 77 | 62 | 78 |
| 11 | ...... | ...... | 54 | 110 | 43 | 49 | 49 | 93 | 49 | 81 |
| 12 | ...... | ...... | 35 | 101 | 32 | 75 | 31 | 74 | 58 | 122 |
| 13 | ...... | ...... | 41 | 80 | 30 | 95 | 42 | 81 | 59 | 71 |
| 14 | ...... | ...... | 39 | 95 | 23 | 133 | 42 | 59 | 44 | 73 |
| 15 | ...... | ...... | 72 | 114 | 49 | 61 | 39 | 68 | 44 | 55 |
| 16 | ...... | ...... | 42 | 110 | 56 | 80 | 33 | 50 | 45 | 80 |
| 17 | ...... | ...... | 46 | 81 | 40 | 58 | 40 | 70 | 44 | 60 |
| 18 | ...... | ...... | 50 | 56 | 37 | 56 | 32 | 62 | 26 | 55 |
| 19 | 140 | 188 | 105 | 136 | 30 | 47 | 38 | 78 | 37 | 58 |
| 20 | 62 | 70 | 45 | 48 | 36 | 74 | 63 | 91 | 36 | 100 |
| 21 | 35 | 144 | 63 | 71 | 56 | 90 | 37 | 60 | 39 | 55 |
| 22 | 40 | 42 | 66 | 82 | 37 | 56 | 43 | 37 | 63 | 104 |
| 23 | 47 | 120 | 67 | 97 | 74 | 103 | 44 | 85 | 63 | 113 |
| 24 | 32 | 109 | 69 | 79 | 44 | 86 | 60 | 64 | 55 | 101 |
| 25 | 70 | 63 | 63 | 136 | 52 | 51 | 44 | 56 | 50 | 64 |
| 26 | 91 | 191 | 33 | 60 | 55 | 59 | 39 | 71 | 35 | 66 |
| 27 | 44 | 51 | 50 | 58 | 75 | 51 | 47 | 80 | 56 | 84 |
| 28 | 33 | 34 | 21 | 101 | 42 | 52 | 41 | 57 | 54 | 107 |
| 29 | 50 | 101 | 62 | 73 | 37 | 60 | 56 | 65 | 55 | 56 |
| 30 | 47 | 63 | 51 | 80 | 56 | 46 | 57 | 131 | 57 | 69 |
| 31 | ...... | ...... | 57 | 151 | ...... | ...... | 53 | 97 | 38 | 54 |
| Total | 691 | 1,176 | 1,592 | 2,929 | 1,538 | 2,605 | 1,386 | 2,309 | 1,479 | 2,275 |
| Mean | 57.6 | 96.0 | 51.3 | 94.5 | 51.3 | 86.6 | 44.7 | 74.5 | 47.7 | 73.4 |
| Hourly mean | 5.8 | 6.8 | 5.1 | 6.7 | 5.1 | 6.2 | 4.5 | 5.3 | 4.8 | 5.2 |

It appears from these results that the average wind movement is exceptionally low. The importance of this fact in its indication of a uniform atmosphere, and in connection with astronomical photography, will be appreciated by astronomers. The shaking of a large instrument by the wind is frequently so serious as to reduce greatly the quality of astronomical photographs obtained in windy weather. At Mount Wilson, where a dead calm is an exceedingly common occurrence, all of the most exacting requirements of astronomical photography are completely realized.

### TRANSPARENCY OF THE ATMOSPHERE.

I have previously alluded to the dust-storms which sometimes enter the San Gabriel Valley through the Cajon Pass from the Mojave Desert, and those much rarer storms in which the dust is carried by the wind completely over the Sierra Madre Mountains. In the more common form of dust-storm (the so-called "Santa Ana") the dust enters the valley in a fairly well-defined mass and proceeds westward along the canyon of the Santa Ana River. In approaching the coast it spreads over a large area and diffuses itself with tolerable uniformity through the lower atmosphere. I have seen from Mount Wilson a dust-storm in the region of Riverside, which in twenty-four hours had spread itself over Los Angeles and Pasadena. When it reached this part of the valley there was almost no wind, and the dust seemed to diffuse itself through the air. Such storms sometimes completely hide the Sierra Madre Mountains from observers in Pasadena. Fortunately they are almost always confined to the lower atmosphere, and do not appreciably affect the transparency of the sky above Mount Wilson, where daily observations show that the transparency of the day and night sky are very satisfactory.

### SEEING.

Systematic tests of the definition of the solar image have been made on Mount Wilson with a telescope of 3¼ inches aperture, with an eyepiece giving a power of about 100 diameters. At first the character of the seeing was rated on a scale of 5; but it soon appeared that a scale of 10 would be preferable under the existing conditions. Accordingly, the seeing as recorded in the following table is given on a scale of 10. Seeing 8, which is so frequently obtained during the early morning hours, represents a sharply defined image of the sun, showing the granulation and the details of the spots with great distinctness, and indicating practically no trembling at the limb. Such seeing occurs at the Yerkes Observatory only occasionally, although that observatory seems to be better situated than many other institutions for work on the sun.

An examination of the table will show that the seeing is best during the early morning hours, although the image is frequently very good in the late afternoon. Shortly after sunrise the sun's limb is serrated, but this effect becomes less and less marked as the sun's altitude increases. Usually, at this time in the morning, the atmosphere is almost perfectly calm and cloudless. The seeing usually

improves and reaches a maximum, where it remains for some time. The effect of the heating of the mountain then becomes apparent and the definition deteriorates. The disturbances at the sun's limb under these conditions do not resemble those seen immediately after sunrise, but have a fluttering appearance, which we are accustomed to speak of as the "heating effect." In the late afternoon the seeing usually improves, but it is rarely very good at midday. This is not a rule without exceptions, however, as we have sometimes recorded nearly perfect definition during the hottest hours of the day.

Everyone who has noted the heated air above the surface of the ground will wonder, in considering the effect of such disturbances upon solar observations, whether these disturbances rise to a great height. A casual observation is sufficient to show that the disturbance decreases rapidly in passing upward from the ground, but it is, of course, quite impossible to determine by means of the unaided eye the probable effect of this disturbance on telescopic observations. We have accordingly made many observations of the sun with the 3¼-inch telescope supported in a pine tree at heights above the ground ranging from 20 to 80 feet. The results of these observations clearly indicate that a telescope employed in solar work should be mounted as high above the ground as circumstances warrant. At the lower elevations in the tree the advantage over positions still nearer to the ground was sometimes not appreciable; but at a height of 80 feet above the ground the improvement in definition was very distinct. Probably this is one of the reasons why the solar definition with the 40-inch Yerkes telescope averages considerably better than we expected it would, for with this telescope the object-glass is over 70 feet above the ground.

SEEING.

| April. | Hour of observation. | | | | | | | | | April. | Hour of observation. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 6 | 7 | 8 | 9 | 10–2 | 3 | 4 | 5 | 6 | | 6 | 7 | 8 | 9 | 10–2 | 3 | 4 | 5 | 6 |
| 1 | — | 5 | 5 | 4 | 4 | 4 | 5 | 4 | — | 16 | — | 5 | — | 5 | — | — | — | 4 | — |
| 2 | — | — | 5 | 6 | 5 | 5 | 4 | 4 | — | 17 | — | — | 5 | 4 | 4 | 6 | 6 | — | — |
| 3 | — | 5 | 6 | 7 | 6 | — | — | — | — | 18 | — | 6 | 7 | — | — | — | — | — | — |
| 4 | — | 5 | 6 | 7 | 6 | 5 | 6 | — | — | 19* | — | — | — | — | — | — | — | — | — |
| 5 | — | 8 | 8 | 8 | 7 | — | 8 | 8 | 6 | 20 | — | 6 | 5 | — | — | — | — | — | — |
| 6 | — | 6 | 7 | 6 | 4 | 7 | 7 | 5 | — | 21 | — | 6 | 7 | 6 | 5 | 4 | 6 | 6 | — |
| 7 | — | 9 | 8 | 7 | 7 | 6 | 7 | 7 | 5 | 22* | — | — | — | — | 4 | — | — | — | — |
| 8 | — | — | 7 | — | — | — | — | — | — | 23 | — | — | 5 | 4 | 4 | 4 | 4 | — | — |
| 9 | — | 5 | 5 | 4 | 4 | 4 | 5 | 4 | — | 24 | — | — | 7 | 6 | 5 | 5 | 6 | 6 | 7 |
| 10 | 6 | 7 | 7 | 6 | 6 | 7 | — | — | — | 25 | — | — | — | — | 4 | 4 | 4 | 4 | — |
| 11 | 7 | 9 | 7 | 6 | 6 | 7 | — | — | — | 26† | — | — | — | — | — | — | — | — | — |
| 12 | — | 7 | 7 | 6 | — | — | — | — | — | 27* | — | 6 | 4 | — | — | — | — | — | — |
| 13 | — | 8 | 8 | 7 | — | — | 7 | 8 | 7 | 28* | — | 6 | — | — | — | — | — | — | — |
| 14 | — | 6 | 6 | 6 | 5 | 5 | 4 | — | — | 29 | — | 7 | 6 | 6 | 4 | 5 | 6 | — | — |
| 15 | — | — | — | — | — | — | — | — | — | 30 | 6 | 7 | 4 | 4 | 4 | 4 | 6 | — | — |

* Rain. † Snow.

| May. | Hour of observation. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 6 | 7 | 8 | 9 | 10–2 | 3 | 4 | 5 | 6 |
| 1* | — | — | — | — | — | — | — | — | — |
| 2 | — | 5 | 4 | 4 | 3 | 2 | 2 | 2 | — |
| 3 | — | 6 | 6 | 7 | 5 | 4 | 6 | — | — |
| 4 | — | 7 | 6 | 5 | 4 | 5 | 5 | — | — |
| 5 | — | 7 | — | — | 4 | 5 | 5 | 4 | — |
| 6 | 7 | 8 | 6 | — | — | — | — | — | — |
| 7 | 5 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | — |
| 8 | — | 7 | 7 | 5 | — | — | — | — | — |
| 9† | — | — | 5 | — | — | — | — | — | — |
| 10 | — | 5 | 6 | 5 | 5 | — | — | — | — |
| 11 | — | 6 | 5 | 5 | 4 | — | — | — | — |
| 12 | — | — | — | — | — | — | — | — | — |
| 13 | — | — | — | — | — | — | — | — | — |
| 14 | — | — | — | — | — | — | — | — | — |
| 15 | — | 5 | 6 | 5 | — | — | — | — | — |
| 16 | 6 | — | — | 7 | 7 | — | 6 | 6 | — |
| 17 | — | — | — | — | — | — | — | — | — |
| 18 | — | — | — | — | — | — | — | — | — |
| 19 | — | — | — | — | — | — | — | — | — |
| 20 | — | — | — | — | — | — | — | — | — |
| 21 | — | — | — | — | — | — | 5 | — | — |
| 22 | — | 6 | 7 | 8 | — | 4 | — | — | — |
| 23 | — | 6 | — | 6 | 5 | — | 6 | — | — |
| 24 | — | 8 | — | 6 | 6 | — | 5 | — | — |
| 25‡ | — | — | — | — | — | — | — | — | — |
| 26‡ | — | — | — | — | — | — | — | 8 | — |
| 27 | — | 8 | 7 | 6 | 6 | 7 | 7 | 8 | — |
| 28 | — | 8 | 8 | — | 7 | — | 8 | — | 7 |
| 29 | — | 9 | 8 | 8 | 8 | — | — | — | — |
| 30 | — | 7 | 8 | 8 | 7 | 6 | 6 | 7 | — |
| 31 | — | 6 | 7 | — | 6 | 6 | 7 | — | — |

| June. | Hour of observation. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 6 | 7 | 8 | 9 | 10–2 | 3 | 4 | 5 | 6 |
| 1† | — | — | — | — | — | — | 6 | — | — |
| 2 | — | — | 7 | 6 | 6 | 6 | 6 | 6 | — |
| 3 | — | 6 | 6 | 6 | 6 | 6 | 6 | 6 | — |
| 4 | — | 6 | — | 7 | 5 | — | — | — | — |
| 5 | — | 8 | 8 | — | 7 | — | 6 | — | 4 |
| 6 | — | 9 | 9 | 8 | 7 | 7 | — | — | — |
| 7 | — | 7 | — | — | 4 | 4 | 6 | — | 7 |
| 8 | — | 8 | 8 | — | 6 | — | — | 7 | 6 |
| 9 | — | 9 | 8 | 6 | 6 | — | 6 | 7 | — |
| 10 | — | 9 | 8 | — | 7 | — | — | 7 | — |
| 11 | — | 9 | 8 | — | 6 | — | 8 | 7 | 9 |
| 12 | — | 9 | — | — | — | — | — | — | — |
| 13 | — | — | — | — | — | — | — | — | — |
| 14 | — | — | — | — | — | — | — | 8 | — |
| 15 | — | 9 | 8 | 7 | 6 | — | 7 | 7 | — |
| 16 | — | 9 | 8 | — | 7 | — | 8 | 7 | 8 |
| 17 | — | 9 | 9 | 7 | 7 | 5 | — | 7 | 8 |
| 18 | — | 8 | 7 | — | 5 | — | 7 | 8 | 7 |
| 19 | — | 9 | 6 | 6 | 4 | — | — | 7 | — |
| 20 | 6 | 6 | — | 6 | 5 | 7 | 7 | — | 6 |
| 21 | — | 6 | 6 | — | 5 | — | 6 | — | 6 |
| 22 | 8 | — | 8 | 6 | 6 | 6 | 7 | 8 | 8 |
| 23 | — | 8 | 7 | — | 7 | — | 7 | 8 | — |
| 24 | — | 9 | 8 | — | 6 | — | 7 | 8 | — |
| 25 | 8 | 9 | 7 | — | 7 | — | 7 | 7 | — |
| 26 | — | 9 | 8 | 8 | 6 | — | 6 | 6 | — |
| 27 | — | 8 | 8 | 7 | 7 | — | — | 5 | 6 |
| 28 | 8 | 8 | — | — | 6 | — | 6 | 6 | — |
| 29 | 8 | 9 | — | 6 | 6 | — | — | 8 | — |
| 30 | 8 | 9 | — | 8 | 8 | 8 | 8 | 8 | 7 |

| July. | Hour of observation. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 6 | 7 | 8 | 9 | 10–2 | 3 | 4 | 5 | 6 |
| 1 | — | 5 | — | 6 | — | — | 6 | 6 | — |
| 2 | — | 8 | — | — | 4 | — | — | — | — |
| 3 | — | 8 | 7 | — | 6 | — | — | — | 4 |
| 4 | — | 6 | — | 7 | 5 | — | — | — | — |
| 5 | 8 | 8 | — | — | 5 | — | 7 | — | 6 |
| 6 | — | 8 | 8 | — | 7 | — | — | 8 | 8 |
| 7 | — | 8 | 7 | — | — | — | — | — | — |
| 8 | 8 | 8 | 7 | — | 7 | — | — | 8 | 8 |
| 9 | 8 | 9 | — | — | 6 | — | — | — | — |
| 10 | — | — | — | — | — | — | — | — | — |
| 11 | — | 8 | 6 | — | 5 | — | 7 | 8 | 6 |
| 12 | 8 | 9 | 8 | — | 5 | 6 | 7 | 7 | 7 |
| 13 | 8 | 8 | 7 | 7 | 6 | — | — | 8 | 7 |
| 14 | 8 | 8 | 7 | — | 6 | — | 7 | 7 | 7 |
| 15 | — | 7 | 6 | 5 | 4 | 4 | 6 | 7 | — |
| 16 | — | 5 | 4 | 4 | — | — | — | — | — |
| 17 | — | — | — | — | — | — | — | — | — |
| 18 | 8 | 8 | 8 | 7 | 6 | — | — | — | — |
| 19 | 6 | 8 | 7 | 7 | 6 | — | — | — | — |
| 20 | 7 | 8 | 7 | 6 | 4 | — | 6 | 8 | 7 |
| 21 | — | — | — | — | — | — | — | — | — |
| 22 | — | 7 | — | 6 | 5 | — | 6 | 7 | — |
| 23 | — | 7 | — | 6 | 5 | — | 6 | 7 | — |
| 24 | — | 7 | — | — | — | — | — | — | — |
| 25 | — | 6 | 5 | 4 | 4 | 4 | 5 | — | — |
| 26 | — | — | 6 | 5 | 4 | 5 | 6 | 7 | — |
| 27 | — | 6 | 5 | 4 | 4 | 5 | 6 | 7 | — |
| 28 | 7 | 8 | 7 | — | 4 | — | — | 4 | — |
| 29 | — | 7 | 6 | — | 4 | — | 8 | 7 | — |
| 30 | — | 8 | 7 | 6 | 6 | — | — | — | — |
| 31 | — | — | — | — | — | — | — | — | — |

| August. | Hour of observation. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 6 | 7 | 8 | 9 | 10–2 | 3 | 4 | 5 | 6 |
| 1 | — | — | — | 6 | 5 | 6 | 7 | 8 | — |
| 2 | 6 | 6 | 6 | — | 4 | 6 | 7 | 6 | — |
| 3 | — | 7 | — | — | 5 | — | 7 | 7 | — |
| 4 | — | 6 | 7 | 7 | 6 | 6 | 6 | 7 | — |
| 5 | — | 8 | 8 | — | 5 | 5 | — | — | — |
| 6 | 8 | 8 | 7 | 6 | — | — | — | — | — |
| 7 | — | 8 | — | — | — | — | — | — | — |
| 8 | — | 8 | — | — | 5 | 6 | 6 | 6 | — |
| 9 | 6 | 7 | — | 6 | 4 | — | 7 | 8 | 8 |
| 10 | 6 | 7 | 7 | — | 5 | 6 | — | 6 | — |
| 11 | 7 | 7 | 6 | — | 5 | 6 | — | 6 | — |
| 12 | 6 | 5 | — | — | 5 | 6 | — | 6 | — |
| 13§ | 6 | — | — | — | — | — | — | 4 | — |
| 14§ | — | 7 | — | — | — | — | — | — | — |
| 15 | — | 7 | — | — | — | — | 8 | — | 6 |
| 16 | 8 | 7 | — | — | 4 | — | 8 | — | 7 |
| 17 | — | 8 | 7 | — | 7 | — | 7 | 7 | — |
| 18 | 8 | 7 | 6 | — | 5 | — | 7 | 7 | — |
| 19 | 7 | 8 | — | — | 6 | — | 7 | 7 | — |
| 20 | 5 | 6 | 6 | — | 6 | — | 6 | 6 | — |
| 21 | — | 8 | 8 | — | 6 | — | 7 | — | 6 |
| 22‡ | — | — | — | 6 | — | — | — | — | — |
| 23 | — | — | 8 | — | — | — | — | 7 | — |
| 24 | — | 6 | — | — | 5 | — | — | 6 | — |
| 25 | 8 | 7 | 6 | — | 5 | — | 6 | — | 6 |
| 26 | — | 9 | 7 | — | 5 | — | 7 | — | 6 |
| 27 | 8 | 8 | 7 | — | 6 | — | 7 | — | 6 |
| 28 | — | 8 | 7 | 6 | 5 | — | 7 | 6 | — |
| 29 | — | 8 | 7 | — | 5 | — | — | — | 7 |
| 30 | — | 8 | 7 | — | 5 | — | — | — | 7 |
| 31† | — | 3 | — | — | — | 8 | — | 6 | — |

* Snow. † Cloudy ‡ Rain. § Storm.

OBSERVATIONS WITH THE FIFTEEN-INCH COELOSTAT TELESCOPE.

In March, 1904, a coelostat of 15 inches aperture was sent to Mount Wilson from the Yerkes Observatory. This instrument had previously been employed by Professors Barnard and Ritchey, of the Yerkes Observatory party, at the solar eclipse of May 28, 1900, in Wadesboro, North Carolina, and by Professor Barnard at the Sumatra eclipse in 1902. As used at Mount Wilson, it is supplied with a second plane mirror, mounted south of the coelostat, and arranged to slide on a north and south track in such a way as to receive the solar rays corresponding to any declination of the sun.

The rays are reflected from this mirror toward the north to a 6-inch photographic objective of 61½ feet focal length, mounted on the extension of the stone pier just above the coelostat. After passing through this lens the rays traverse a long tube built of wooden framework and covered with paper. The solar image is formed within a small house which terminates this tube at its north end. In the house a photographic plate-holder is mounted, in conjunction with a slide containing a narrow slit, which can be shot at high speed across the solar image by means of a spring. In this way the very short exposure required for direct photography of the sun can be obtained.

One of the chief points of interest connected with this instrument is the effect of the heating of the air within the tube upon the definition of the solar image. In the first experiments with this apparatus, the skeleton tube was covered on all sides with tar-paper, just as it had been used in the eclipse work. Above the tube, and separated from it by a considerable air-space, was a canvas fly for the purpose of shielding the tube from the direct rays of the sun. It was found that in the early morning, before the tube had become heated, the definition of the solar image was excellent. In a short time, however, heated air within the tube completely spoiled the definition, and the sun's image became so blurred and indistinct that no observations of value could be made with it. These circumstances led us to question what the effect would be if no tube were employed. The 6-inch lens was therefore mounted in such a position as to throw the beam horizontally through the air toward the north, outside of the tube and over that portion of the ground which was in shadow. The image observed under these circumstances was found to be much better defined than that seen through the heated air of the tube. We accordingly decided to try the experiment of taking off all of the paper on the two sides which formed the upper half of the tube.

It also seemed advisable to stretch the canvas fly at a much greater distance from the tube and to provide means of exit at the top for any heated air which might be found under the fly. As soon as the tube and fly had been rearranged in this manner a great improvement was immediately noticed. The definition of the image became much better and the deterioration observed in the previous instance was no longer seen. The air in the tube remained cool, whereas before it had become greatly heated.

These experiments would seem to throw some light on the question of designing suitable tubes and shelters for telescopes used in a horizontal, or nearly horizontal, position. It seems likely that if the coelostat and the instruments used with it could be mounted on piers at a height of 70 feet or more above the ground, it would be unnecessary to use any tube, particularly if the ground below the path of the beam were shielded from the sun by a light canvas cover, stretched at a height of several feet above the surface and suitably ventilated. Of course, the practical difficulties in such a construction are very considerable, on account of the great cost and the lack of stability of high piers. For the Snow telescope it therefore seemed advisable to design a special form of house, in the hope of securing good definition with a solar beam at a moderate height above the ground. Experiments made with the 15-inch coelostat seem to show that this latter instrument is too near the ground for the best results, although it gives excellent definition in the early morning, before the heating of the soil is very great.

The design of the house now under construction for the Snow telescope will be described in a subsequent report. It may be said here, however, that it consists of a skeleton frame of light steel construction, provided with a ventilated roof. The floor is to be of canvas, tightly stretched at a height of one foot above the ground and permitting a free circulation of air below. The inner walls of the house (which is 10 feet wide at its narrowest point) are to be of light canvas, so arranged that they can be raised or lowered at will. The outer walls of the house are to be covered by canvas louvres, so arranged as to shield the entire house from the direct rays of the sun, and permitting a free circulation of air. The stone pier, 27 feet high, on which the coelostat will stand, is also to be shielded from the sun by canvas louvres. The ground surrounding the instrument is fairly well covered with bushes, and the few bare spots can be covered with stretched canvas, if necessary.

*Spectroscopic Observations.*—The spectroscope used with the coelostat telescope is of the Littrow form—a single lens, of 4 inches aperture and 18 feet focal length, serves at once as collimator and camera lens. After passing through the slit, which is mounted in the focal plane of the photographic objective employed with the coelostat, the rays pass to the 4-inch objective, by which they are rendered parallel. They then meet the 4-inch Rowland plane grating, having 14,438 lines to the inch, from which they are returned through the 4-inch objective. The image of the spectrum is formed on a photographic plate, mounted in the focal plane and a little to one side of the slit. This apparatus is giving excellent definition, surpassing that of any spectroscope employed at the Yerkes Observatory.

The character of the results obtained with this spectroscope, and its convenience of manipulation, illustrate one of the arguments in favor of fixed telescopes of the coelostat type, as contrasted with moving equatorial telescopes. At the Yerkes Observatory it has never been possible to attach a sufficiently long and powerful spectroscope to the moving tube of the 40-inch refractor. Such a spectroscope must be mounted in a fixed position on substantial piers, and the telescope must be so constructed as to permit a sharp and well-defined image of the sun to be maintained in a fixed position on the slit. This can readily be accomplished with the aid of a coelostat, provided only that the difficulties peculiar to this type of telescope can be overcome. From the experiments so far made, we believe that the difficulties can be surmounted and that the fixed telescope is certain to become an instrument of great importance in the future.

## CONCLUSION.

From the observations given in this paper, it appears that Mount Wilson meets in a very remarkable degree the requirements of a site for a solar observatory. Indeed, I know of no other site that compares at all favorably with it. If a large solar observatory were established there, it might be expected to yield many important results, not to be obtained under less favorable conditions.

# THE SOUTHERN OBSERVATORY PROJECT.

By Lewis Boss.

The object of this application is to petition for a favorable expression on the part of the Executive Committee in relation to the general plan herein proposed, and especially in relation to the project for observations in the southern hemisphere. This work I should like to take up actively within two, or, at least, three years from the present time.

In my original application to the Trustees of the Carnegie Institution, January, 1902, I briefly outlined the course of the research in behalf of which I petitioned for aid. This is to remind the committee of a special feature of the program then outlined.

Briefly stated, the objective point of my general investigation is to find out what the motions of the stars really are, and, as far as possible, what they mean. Specific things to be investigated are:

(1) The direction and velocity of the solar motion in space to be determined with far more accuracy than they are known.

(2) To investigate the subject of "star streams"—swarms of stars moving in a common direction like meteors—a new subject to which my attention has been specially attracted.

(3) To determine with accuracy the relative distance of various orders of stars—a thing which can certainly be done.

(4) To determine the constant of precession more accurately than it is now known; and generally to examine other questions that may arise.

First of all, the motions must be accurately known, as the basis of the investigation; and this is by far the most laborious part of the work—almost the whole of it, in fact. Grants from the Carnegie Institution enable me to carry on this work with vigor. We are determining the motions from all available material, and before the close of 1905 expect to have results for 5,000 of the more frequently observed stars. My various letters of application and annual reports outline the character of this work.

The value of these results, and of the final discussion, will depend upon the systematic accuracy of these determinations of motion, and upon having a good determination of motion for each star. Both these requirements call for further special observations at the present time.

In the first place, we need a new determination of the positions of standard stars distributed from the north to the south pole of the heavens. In response to my previous application such observations with the meridian circle at Albany have already been approved and will shortly be undertaken. The required alterations in the instrument are nearly complete. This series should be completed within eighteen months, or, at latest, within two years, from the present time. After that I should like to take this instrument at once to some favorable station in the southern hemisphere for the observation of standard stars out of reach from stations in the northern hemisphere. The plan is to interlock the two series according to a special plan of mine designed to bring about elimination of systematic errors of observation, by making them work in opposite directions in the two opposite positions of the instrument.

In the second place, we need at the present time special observations of stars that have been neglected for the past twenty or thirty years. We must bring up the accuracy with which these motions can be derived as nearly as we can to equality with that for the general run of stars. There are very great contrasts in the amount of available observations upon different stars. For stars situated in the southern one-fourth of the sky not more than 30 per cent have been accurately observed since 1880, and very few indeed since 1894. Therefore, I strongly desire to observe all the stars down to the seventh magnitude in the one-fourth of the celestial sphere nearest the south pole.

In this connection I would respectfully refer to the Report on Southern and Solar Observatories in the second Year Book of the Carnegie Institution, and especially to pages 28 to 31, under the caption "Fundamental Meridian Observations," and to pages 108 to 143, containing letters from various astronomers commenting on this part of the program for the Southern Observatory. It will there be seen that these astronomers almost unanimously regard this section of the work (precisely the subject of this application) as the most important part of the program for the proposed Southern Observatory.

My wish would be to take personal charge of this work, but not to remain continuously in the southern hemisphere. My plan would be to organize the work, and remain at the station for nearly one year in the beginning, in order to secure smooth running of the observations, with the desired rapidity of execution and accuracy in the results. My presence for a few months at the end would

probably be desirable, in order to see that no requisite point shall have been neglected before abandonment of the station.

The Dudley Observatory would furnish its transit circle and accessories, the essential point in the plan being the use of the same instrument in both hemispheres. This instrument is one of the finest of its kind in the world and has been used here until its peculiarities are well understood. Moreover, the graduation errors of its circles have been determined through a diligent investigation in which the labor of four persons was employed for a total of more than a year—the most thorough investigation of the kind on record. The effect of this is greatly to increase the accuracy of the instrumental results.

I have in mind two locations, either of which might possibly answer the purpose. The first is San Luis in Argentina, about halfway between Buenos Ayres and the Andes. This was highly recommended by Mr. Davis, chief of the Argentine meteorological service, as a station for a southern observatory. The second is Bloemfontein in South Africa, which was very highly recommended by Sir David Gill as a suitable station for the proposed Southern Observatory. Last year's investigations showed that Australian stations could only be thought of as a last resort.

The plan here proposed is one section of my plan as outlined in my original letter of application to the Institution, and the only section calling for a large annual expenditure. The excellence of the result from the general investigation will depend in a large measure upon the execution of the section of the work to which this application relates.

# METHODS FOR PROMOTING RESEARCH IN THE EXACT SCIENCES.

CONTENTS.



---

Copies of the following letter of Dr. Simon Newcomb, in which he explains his views of the "method by which the Carnegie Institution can best promote research work in the exact sciences," were sent to several prominent scientific men. A number of the replies which were received follow Dr. Newcomb's letter.

[*Letter of Simon Newcomb.*]

WASHINGTON, D. C., *May 12, 1904.*

The following is a brief summary of views which I have at various times expressed to officers of the Carnegie Institution or made known to the public. They embody my well-matured opinion as to the method by which the Carnegie Institution can most effectively promote research in the exact sciences. I begin by setting forth the main features of the situation.

I.

The nineteenth century has been industriously piling up a vast mass of astronomical, meteorological, magnetical, and sociological observations and data. This accumulation is going on without end and at great expense in every civilized country.

The problem of working out the best results from these observations is one which is not being effectively grappled with. The best methods of attacking the problem are little known to investigators in general, being scarcely developed in a systematic form. The result is that what has been done toward obtaining results consists largely in piecemeal efforts by individuals, frequently leading to no well-established results.

Another feature of the situation is the gradual extension of the principles of exact science into the biological and sociological field.

It is through this extension, rather than through adding to the already accumulated mass of facts, that progress is most to be hoped for in the future.

II.

A consideration which I wish most respectfully to urge upon the Institution is the great advantage which comes from mutual discussion and attrition between men engaged in contiguous fields of work. My own work would have been much more effective could I have enjoyed this advantage more fully, and I am profoundly impressed by the waste of labor shown in an important fraction of current scientific researches through the authors not being acquainted with the best methods of work.

III.

Under these conditions it still seems to me, as it has almost from the day the Institution was founded, that the most effective way in which it can promote research in exact science is by the organization of an institute or bureau of exact science in general. If I had only my special field in view, I might suggest simply an astronomical institute; but it seems to me that this would be too restricted to get the best and most desirable results. I can not but feel it most important that exact methods should be extended into other branches of science than astronomy.

In defining the field of work in such a bureau or institution a division of physical and natural science into three great fields may well be borne in mind. One of these fields is that of the old-fashioned natural science, which is concerned very largely with morphology, physiology, and vital processes which do not admit of reduction to mathematical forms.

Another field is that of purely experimental science.

The third field which really needs development is that of observation, which I propose shall be now occupied. The work required is, in brief, the development of mathematical methods and their application to the great mass of existing observations. Doubtless suggestions as to experiment would frequently come in. These would be carried out by others.

IV.

*The Organization.*—The first requirement for the organization is a managing head in whom the Institution has entire confidence, who should be required to devote all his available energy to the work,

and in doing so should act as the agent of, and be regarded as doing the work of, the Carnegie Institution. He should be supplied with such office, appliances, and assistants as are necessary to commence work in that branch of the field with which he feels himself most conversant, beginning work on a small scale, to be enlarged and extended into neighboring fields as success became assured. The opposite faults of beginning on too large a scale and of making no provision for possible expansion should both be avoided.

V.

The head of the institute should be aided by a council comprising the leading experts best qualified to advise as to the various departments of work. This council might be an international one, and, if the work of the institute is sufficiently expanded to justify it, should hold an annual meeting.

In order to secure the advantages of mutual consultation, attrition, and coöperation, it may eventually be desirable that the work the Institution has already undertaken or is now promoting in the various branches of exact science should be merged with the proposed institute.

VI.

The institute should be started on a very modest scale. The case is one in which everything depends on correct methods from the beginning. By the adoption of these, results may be reached at small expense which, without them, would never be reached with any amount of labor. It seems to me that $10,000 or $15,000 would be ample for the expenses of the first year, as the number of employees who could be successfully put to work would be small. The principal appliances required would be books, but I think that three or four office rooms would suffice for all the purposes of the first year or two.

The expenses of subsequent years would depend upon the expansion which it found desirable to give to the work.

Appended hereto are letters on the subject from Prof. H. H. Turner, of Oxford, and Lord Rayleigh, to each of whom I presented the question of the desirableness of working up the great mass of observations alluded to.

SIMON NEWCOMB.

[*Letter of H. H. Turner.*]

UNIVERSITY OBSERVATORY, OXFORD,
*November 25, 1903.*

I have delayed answering your letter of October 30 for a few days, not from any lack of sympathy with its general purport or doubt as to the value—the immense value—which such a scheme as you suggest would have, but because I wished to think whether I could contribute anything of possible importance to the discussion of details. The result has, however, not been very encouraging, and I must not delay longer a reply on the main point.

I imagine you will not find any one to doubt the necessity of a far more extended discussion of results. In the days of Newton perhaps observations were scarcer than theories, and it was advisable to set them going; but, once set going, inertia has come into play here as elsewhere, and observations of all kinds are churning out masses of observations which no one is attempting to deal with. There is no doubt whatever that it is a crying necessity that we should organize the *discussion* of the masses of accumulated material. The necessity extends beyond astronomy—to meteorology certainly; to natural history perhaps, though here the observations (*metrical*) are also needed, as in astronomy in Newton's time.

How, then, to set to work to improve matters? I have no better plan than yours. Perhaps I should approach the subject from rather a different point of view. I should start with the proposition that the amount of critical discussion (*i. e.*, discussion of any value) of results obtained is likely to depend roughly on the number of men of first-rate ability who can be enlisted into the service. For making observations a moderate ability may suffice, but there is no doubt about the ability required for discussing them and directing future programs. Well, then, I fear it must come to this: That we want more *positions of eminence*—well paid or honored or both—such as the leading professorships. When Schuster gave his address, which you quote with approval, Dr. W. N. Shaw (head of our Meteorological Office) remarked that meteorology had never had any *professorships* at the universities (Is this also true in the United States?), and I think the remark went very near to a sufficient explanation of the lack of adequate discussion of results. You can get heaps of people to measure rainfall, but who is to think about the results? It is more thinkers we want.

Hence my proposition comes to this: Either—

(1) Endow more really first-class posts, such as will attract good men. It is no use getting youngsters into the science unless there is some prospect for them; or,

(2) Look about for means for drawing into the work of discussion occupants of existing positions of repute who are now either wasting their time accumulating little-needed observations or are prevented from doing such work by the lack of machinery—*i. e.*, of funds for getting computing done—for there is a good deal of computing attached to most discussion of masses of observations.

One could accordingly meet the present need in a variety of ways. When you were over here I was speaking of a "calculating bureau" (and you seemed to approve). This would follow from the second part of No. 2. If a man (like Sampson or Durham) knew that he could get computing done pretty easily if he would arrange the details, he might be rendered efficient when otherwise his way would be blocked. The relief might be compared to that afforded in the matter of *printing and publication* which our societies have afforded and which the American observatories are finding in their "bulletins" and "circulars." Before printing was easy much good work must have been lost.

But this is only one way of meeting the need and is practically included in your method, which includes, indeed (if I understand you rightly), all the elements I have sketched. At the head of your suggested organization you could scarcely fail to have at least *one first-rate man*, which so far meets my point 1. You virtually meet the first part of 2 by establishing, instead of a new observatory to multiply observations, an organization of a new kind, which will set a good example to others, and the rest of 2 I have already considered. I have written truly my thoughts as they occur, and hope this letter is not too long and rambling. One can not help, when these inspiring letters talking of new projects come from over the water, building a few castles in the air. One of my castles is a really critical astronomical journal, for discussing the work of others rather than publishing our own. To some extent the V. J. S. does this, but we could do with an English journal of the kind, and a better one. If you get your way perhaps this journal might be tacked on to the scheme.

H. H. TURNER.

[*Letter of Karl Pearson.*]

UNIVERSITY COLLEGE, LONDON, ENGLAND,
*June 24, 1904.*

DEAR SIR: I have put together a few suggestions that occur to me, principally based on my own personal experience; but I do not wish them to be considered in any way as dogmatic statements, only as impressions.

(1) I agree absolutely with Professor Newcomb's first statement that the nineteenth century has industriously piled together a vast mass of astronomical, physical, and biological data, and that very little use has hitherto been made of this material. The reason for this I take to be that a man of mediocre ability can observe and collect facts, but that it takes the exceptional man of great logical power and control of method to draw legitimate conclusions from them.

(2) Differing probably from Professor Newcomb, I hold that at least 50 per cent of the observations made and the data collected are worthless, and no man, however able, could deduce any result from them at all. In engineers' language we need to "scrap" about 50 per cent of the products of nineteenth century science. The scientific journals teem with papers which are of no real value at all. They record observations which can not be made of service by any one, however able, because they have not been undertaken with a due regard to the safeguards which a man takes who makes observations with the view of testing a theory of his own. In other cases the collector or observer is hopelessly ignorant of the conditions under which alone accurate work can be done. He "piles up" observations and data because he sees other men doing it and because that is supposed to be scientific research.

(3) I have had to deal to a great extent with the observations and data of other men in my statistical laboratory, to which applications are always being made for aid in the interpretation of observations. I think I might help to illustrate my point by citing a few actual experiences.

(*a*) *Meteorological Statistics.*—We have here a large work in progress. The data are enormous, but without any system. Examination shows that in Europe and America the returns are often untrustworthy. There is no standardization of method, of time, or of quantity observed. Important stations are omitted or dropped for years, and where a well-organized plan for a quarter of the expense and labor would have led to definite results, the existing chaotic

mass of data will only provide probabilities and suggestions. Any man with ideas on the subject of meteorology would after a little experience discard existing material and start afresh, or else waste his best years in trying to reduce material to a common measure, which is really a hopeless task.

(*b*) *Medical Statistics.*—These are made by each medical man and each hospital on a separate plan, and without any idea, as a rule, of the points which it is needful to observe in order that logical conclusions may be drawn. This is especially the case in inheritance of disease tendencies. Further, immense masses of material are wasted because one or other essential factor has escaped record in one or other series.

We have had to report recently on cancer statistics, lunacy statistics, and inoculation for enteric fever statistics. Only moderately definite conclusions can be drawn, because the material has usually been collected without insight into the conditions requisite for drawing definite statistical results.

(*c*) *Physical Measurements.*—The same applies here, in perhaps a less degree, but still quite definitely. Observations on the strength of materials exist in immense quantities. These are largely of no value because the experimenters have had no clear idea *a priori* of the points they wanted to elucidate. Further, this applies to a whole mass of physical observations which have been made without sufficient mathematical knowledge to realize the difficulties of the problem. The failure on this account of physicists like Wertheim, Savant, and Kupffer in the first half of the nineteenth century is quite paralleled in recent work by men whom for obvious reasons it is better to leave unnamed.

(*d*) *Biological and Sociological Observations.*—These are of the lowest grade of value in too many cases. Even where the observers have begun to realize that exact science is creeping into the biological and sociological fields they have not understood that a thorough training in the new methods was an essential preliminary for effective work, even for the collection of material. They have rushed to measure or count any living form they could hit on without having planned *ab initio* the conceptions and ideas that their observations were intended to illustrate. I doubt whether even a small proportion of the biometric data being accumulated in Europe and America could by any amount of ingenuity be made to provide valuable results, and the man capable of making it yield them would be better employed in collecting and reducing his own material.

It will be seen from the above results that I personally can not form a very high expectation of the amount of results of first-class value which would be obtainable by forming an institute to deal with the existing masses of observations.

(4) Nevertheless, if we reject 50 per cent of existing observations as worthless, if we frankly "scrap" them, I still think something of service might be done with the remainder under certain conditions.

(*a*) If the right man were available. This is the chief difficulty. He must be a man of wide appreciation of many branches of science, otherwise a special man will be wanted for each branch—astronomy, meteorology, physics, medical science, sociology, etc. Even were the money forthcoming for this multiplicity of workers, I doubt whether the men themselves are to be found. If Professor Newcomb's institute is carried out, the right man for director will be a man of very exceptional attainments, falling little short of scientific genius. I doubt if one man of this type could be procured. It is certain that several could not.

(*b*) The right man must have been rightly trained. He is to be occupied in drawing logical conclusions from other persons' observations and data. He must therefore in the first place be an adept in scientific method; he must be a first-class mathematician, statistician, and a trained calculator and computator.

(*c*) The right man must be rightly supported. He must have a competent staff of workers under him, and be to a considerable extent a man of affairs. He will have to reject after examination whole masses of observations and data as unsuitable, and his proceedings will be questioned and criticised. Unless he is a man of weight and tact, he will soon be in an impossible position relative to the mediocre observers whose data he is to manipulate. For example, he proposes to deal with the weights of the human viscera in health and disease. He collects all the available data, but issues his report and conclusions, silently passing by the measurements of some well-known physician or hospital, because they have been made in a manner which renders them of no real scientific value. The result would be certainly controversy, possibly uproar, and the director of the institute would have to fight a series of pitched battles before his reputation as a censor and official "scrapper" was finally established beyond dispute. He might survive this initial state of affairs if he had the support of the best scientific minds in the country; but unless he was a strong man he would take the easier course, and simply add another long series of reports on *all*

existing material to the already overvoluminous scientific literature of the day. The right man will be the man who has the courage to "scrap" and to do it relentlessly. Science wants immensely the courageous pruner to-day; but his is not an enviable task, and the Carnegie Trustees would have to support their man pretty steadily to enable him to be effectual. He will be sure to make some mistakes, and these will be at once seized on and trumpeted abroad. If we suppose that the above three conditions can be fulfilled, may we not question whether the man pictured would not be of such caliber that he would do far better work for science if he were allowed to use other people's observations where he chose, and to observe and collect himself where he found them defective or incapable of throwing light on the branches of science he was peculiarly interested in? In other words, the director would be reduced to an ordinary scientific worker, placed in one sense under very favorable conditions, in another under unfavorable conditions; he would have ample material and support, but he would differ from an academic teacher in having no school wherein he might train his subordinates in his methods.

(5) On the whole, I doubt whether the founding of an institute to "scrap" and codify existing observations and scientific material is feasible if desirable. I am inclined to think that more might be done by a *Statistical and Computating Institute*. This institute should have a competent director and a highly trained staff. It should be prepared to report on any data or material submitted to it at a moderate fee. This fee might be remitted on the recommendation of the director, or a committee, in the case of first-class work from a man of scientific repute but small means. It would have to be retained, however, to prevent a flood of worthless material being sent in to be reduced. The institute might also offer advice on the collection of material on observational method and on statistical treatment, again charging a slight fee to prevent the institute being used as a source for providing research work for those who were too idle or too dull to discover such work for themselves. Besides, private individuals, learned societies—astronomical, meteorological, or biological—might and probably soon would use the institute to carry out special investigations on the value of material already amassed in some one or other branch of their special sciences. Finally, Government departments would very soon fall into the habit of asking for reports on the special material of their own spheres. The like course would be taken by local bodies in the case of demographic

and other statistical material. I think that such an institute would be of very great service, and, perhaps as far as possible, fulfill the functions which Professor Newcomb proposes, without the great amount of friction that a direct inquiry into the value of material collected by men, many of whom would still be holding scientific posts, would certainly involve.

Of course one is far too apt to judge matters from one's own little corner of the field of science. We have had a statistical laboratory established for some little time, and we find that an increasing number of workers send us their data for suggestion and report. To such an extent has this become current that we shall probably have either to institute a fee to check the flow of material or else decline to examine such work, as we are only an academic department, doing our own teaching and research work, and without public support of any kind. Still our small experience may be useful on the other side of the Atlantic ; and we have found a multiplicity of workers, physical and biological, want assistance, and further that public bodies and government departments seek statistical and calculating aid also. If Professor Newcomb's ideas were carried out first on material which was actually placed before the institute for report, then the action of scientific societies and public bodies would soon give the foundation an established position, from which possibly the more serious business of codifying and "scrapping" existing accumulations of observations and data could ultimately be carried out without too great friction and controversy.

KARL PEARSON.

---

[*Letter of Lord Rayleigh.*]

ROYAL INSTITUTION OF GREAT BRITAIN,
*November 20, 1903.*

DEAR PROFESSOR NEWCOMB :

I am in complete sympathy with the views expressed in your letter of October 30, and have indeed sometimes expressed myself in a similar sense ; but my experience is far less than yours.

I sincerely hope you may succeed in organizing such an establishment as you indicate.

RAYLEIGH.

[*Letter of G. H. Darwin.*]

NEWNHAM GRANGE, CAMBRIDGE.

I sympathize very warmly with Professor Newcomb's plan for developing the Carnegie Institution and think that it may have a great future. I have been trying to picture to myself how it would work out, and I see that while the gain in some subjects would be great and immediate, in others it would be only collateral.

Scientific observations may be roughly classified in two groups, which, however, graduate into one another. I can best illustrate my meaning by examples.

The subject of the tides seems to belong to the group which would reap immediate advantage. Observations are now published in the most diverse places and are not properly coördinated. A critical collection of tidal results would be a heavy task and would be of much value. There is nothing in this subject which corresponds to probable error in astronomy, for the defects depend on human frailty. It would require a first-rate man to classify and reject observations according to the internal evidence afforded by them. When such a collection was made, generalizations would follow, and the value of the conclusions would probably be great.

Meteorology and many other subjects fall into this group. The distinguishing feature is that we know exactly what to observe, that the mass of material is already enormous, and that it is impossible to have too much matter, provided that it is coördinated.

The second kind of research to which I have referred is intermediate between observation and experiment. The subject of observation is to some degree indeterminate, and it depends on the investigator what he shall observe.

I can not think of a very good example at the moment, but I may perhaps illustrate my meaning by supposing that we were investigating the laws governing the drifting of sand and the formation of sand dunes. It must be obvious that this is a subject of great agricultural importance in many parts of the world. Now, it would be almost useless merely to collect maps and photographs. There must be a guiding mind, forming theories to be proved or disproved by observation. The investigation might be expensive and troublesome, but it is essentially the work of an individual.

In this sort of case I should not look for any great gain from the proposed institution, except that it would afford a fixed position, with good pay, to men of ability. The exception is important, and it

brings us to the point raised by Professor Turner, viz, that the search for men is more difficult and more important than the search for facts.

I hope that you will not regard this long letter as wide of the point, and in conclusion I desire to express my warm approbation of the scheme. G. H. DARWIN.

---

[*Letter of Arthur Schuster.*]

KENT HOUSE, VICTORIA PARK, MANCHESTER,
*August 18, 1904.*

In answer to your request to have my views on the letter addressed to you by Professor Simon Newcomb, I will take his various points in order :

I. There can be absolutely no doubt on the correctness of Professor Newcomb's view regarding the piling up of a vast mass of observations, which has been made an object in itself, instead of being a means to an end, and hence a proper discussion has not been able to keep up with the accumulation of undigested figures. The efforts of individuals to discuss results have often been hampered by want of assistance or of funds, and in many cases have been doomed to failure owing to the fact that the men trained to observe are very often not particularly well fitted to draw conclusions. It would be easy to find examples of the waste of labor which has resulted from incompetent work in the planning out of the methods of reduction.

II. Here also I agree with Professor Newcomb, and I would like to add another feature of the present situation which stands in the way of the discussion of great problems on a broad basis—the vast mass of accumulating material has rendered it necessary to have a special journal almost for each special branch of a subject ; thus we have a journal dealing with solar physics, and another with terrestrial magnetism, etc.

The mathematician and physicist who is probably most capable of dealing with the problems of solar physics and terrestrial magnetism often never sees these journals. If he does he will get bewildered by the mass of detail which is put before him, and often by technical terms which he does not understand.

What is required here is some intermediate agent whose business it should be, on the one hand, to place before the man of general science the main results of observations which want discussing, and on the other hand before the observer the main facts and measurements which the theoretical student requires for his work.

The efforts which have been made to remedy this recognized difficulty by the publication of abstracts have, in my opinion, proved failures. To write efficiently an abstract which would give the pith of a paper in a form that can be utilized requires a very intimate knowledge of the subject. In a subject requiring special skill and training this can not be expected from those who at present undertake work of this kind, nor is the frame of mind of the reader who takes up one of these journals of abstracts and endeavors to assimilate in half an hour the ideas of one hundred and fifty different workers on one hundred and fifty different subjects such as to make it likely that his thoughts will be usefully fertilized. A much more useful plan would be to have periodical reports dealing with the progress of the subject; but here again all will depend on how far it would be possible to get men who thoroughly understand the subject to write these reports.

It is doubtful to my mind whether the best results ever can be obtained by an observer who has not full grasp of what his observations will be used for; but, dealing with the question from a practical point of view, we must recognize that there are many men who can take excellent observations without any special power of discussing them, and it would be a pity not to make use of such men, provided we can convince them of the limitation of their powers.

III. An institute or bureau of exact science, according to Professor Newcomb's scheme, would, in my opinion, prove useful, as it might in each subject find the best methods of coördinating facts and reducing observations; but the organization of the bureau would have to adapt itself to the different requirements of the different subjects, these requirements probably varying from time to time. In particular stages of a subject publication of a list of papers may be what is required, and in every case we must guard against stereotyping any one particular method of procedure. The abstracts which, as above mentioned, I found useless in my own subject might be very effective in others.

It would be, as Professor Turner points out, a very material gain if there were a body of men whose special duty consisted in discussing observations and drawing attention to those matters where observation is most required. I consider the subjects included in Professor Newcomb's third "field" as requiring most attention at the present moment.

The bureau should, in my opinion, not only have power to initiate reductions, but should also be able to assist other workers in cases

where its council approves of the proposed method. I may mention an example from my own experience. I have engaged during the last two years, at my own expense, an assistant to do certain reductions of sunspot observations by a method which, I believe, will give useful results in many branches of cosmical physics. It would have been advisable in any case that the first set of reductions by this method should have been carried out under my own supervision, but supposing the results arrived at to be valuable and the method to commend itself to competent judges, it would be quite beyond the powers of any individual to extend the calculations so as to include other phenomena, such as prominences or magnetic disturbances, which can be brought into connection with sunspots. The bureau, with funds at its disposal and a committee of directors who could judge of the value of any proposed piece of work, might prevent a block in the advance of science which is at present possible for want of a proper organization.

IV and V. I quite agree that everything must depend on the nomination of a managing head, although an advisory committee will probably be necessary, and it can only be through the organizing powers of a man who is at any rate thoroughly qualified in one branch of science that the work can succeed.

VI. I also agree that the institute should be started on a modest scale. If it is desired that the council should be international, I would suggest that the International Association of Academies should be asked to nominate a certain number of its members. As this association has been founded for the purpose of international coöperation, it seems desirable to strengthen it as far as possible and to avoid the multiplication of other international organizations. I do not, however, wish to express an opinion at present on the desirability of starting the bureau at once on an international basis. It might be better to secure greater elasticity by leaving it, in the first instance, to be an American institution. If desirable, it will always be easy in a few years' time to ask the International Association of Academies to nominate members on its council.

I am sorry there has been so much delay in sending you this reply, but, as I have already informed you, I was unusually busy when your letter reached me.

ARTHUR SCHUSTER.

[*Letter of Edward C. Pickering.*]

HARVARD COLLEGE OBSERVATORY, CAMBRIDGE, MASS.,
*July 27, 1904.*

DEAR SIR: Your letter inclosing a copy of that of Professor Newcomb and requesting a reply before August 1 duly reached me. The plan in general meets with my hearty approval. There is no doubt that a proper discussion of existing observations is very much needed. This should be followed by suitable observations in order to supply the wants thus rendered evident.

To select subjects for the proposed institution a permanent council might be needed, but when a subject was chosen specialists in that department of science should be employed, who would spend several days together arranging the details of the work. According to my experience, a discussion of generalities by a committee with no means at their disposal is unsatisfactory and the results are of little value. A number of experts, however, having an appropriation which they could expend on work with which they were entirely familiar could get much better results than any one person alone. The officer in charge of the proposed institution, with his corps of computers, could readily carry out the plan of work recommended, consulting the committee when difficulties arose, or calling other meetings as required. A large part of the laborious work involved in discussing an extensive series of observations in any department of science could be done to great advantage by such a permanent computing bureau.

It is often impossible to transplant a man of genius in other surroundings without greatly diminishing the value of his work, and it is better to improve his existing conditions rather than try to make him adopt new ones. On the other hand, he is often unable to discuss his own results or supervise large routine computations as well as one who devotes his life to such work. My views on this subject are given more fully in a pamphlet entitled "The Endowment of Astronomical Research, No. 2," which will be distributed in a few days.

EDWARD C. PICKERING.

# FUNDAMENTAL PROBLEMS OF GEOLOGY.

By T. C. Chamberlin.

Sir: I have the honor to submit herewith a report of progress on the work done under Grant No. 115, in continuation of Grant No. 31.

For the general scheme of the work I beg to refer to my previous report (Year Book No. 2, pp. 261–270). The work upon which I have been engaged during the current year has lain wholly within the lines there sketched and chiefly within the constructive phases of the scheme. On the critical side, however, I have reviewed the tests previously applied to the Laplacian and allied hypotheses of the origin of the earth, but have added little to them. The cogency of their adverse bearings seems to be in no way diminished by reflection or reconsideration.

I have developed into more definite terms several phases of the meteoritic hypotheses of the earth's origin of the type advocated by Lockyer and Darwin; that is, the type in which the meteorites are supposed to be assembled as a swarm, the individual meteorites moving to and fro and frequently colliding after the manner of the molecules of a gas, a constitution brought into clear definition by the classic paper of Darwin, "On the Mechanical Conditions of Swarms of Meteorites and on Theories of Cosmogony."* Working upon the results reached by Darwin, it has not appeared probable that at a position so deep in the postulated swarm as that at which the earth should have been formed, a passage from the quasi-gaseous into the true gaseous condition could have been escaped, because of the frequency and violence of the collisions and the consequent high temperature; and hence, so far as the origin of the earth is concerned, this phase of the meteoritic hypothesis seems to become identical with the gaseous or Laplacian hypothesis and to be obnoxious to most of the objections to that hypothesis that arise from the kinetic action of the gases and from the relations of mass and momenta, as brought out in the previous studies by Dr. Moulton and myself.

Studies in the line of meteoritic swarms have usually started with the swarms organized, and have not seriously considered whether such swarms would be likely to arise. There is no positive proof of the present existence of meteoritic swarms with such a dynamic organization. There are, to be sure, spectroscopic and other grounds

---

* Phil. Trans. Royal Society, 1888.

for believing that some nebulæ are composed of discrete solid matter, but it has not been shown that this has a quasi-gaseous organization. For the purposes of a critical discrimination it is necessary to find grounds for supposing that this discrete solid matter is organized as a swarm characterized by heterogeneous movements involving collision and rebound in gaseous fashion, as distinguished from revolutionary movements controlled by gravitation and inertia in planetary fashion, which constitutes the planetesimal organization. The two modes of organization are very distinct dynamically, though they are likely to be more or less combined in any actual system. I have given some time to a study of the possibilities of the origin of such a quasi-gaseous assemblage of meteorites. The studies have taken two lines—(1) the possibilities of assemblage from a primitive diffuse condition, and (2) the possibilities arising from the dispersion of some previous body.

(1) Inspection of the problem made it clear that a grave difficulty lies in the high ratio of the moving force to the gravitational force in celestial bodies, on the average. The gravitational force is obviously the chief agent to be assigned the work of bringing together and holding together the meteoritic swarm in question, while the moving force is the chief opposing or dispersing agent. The gravitative power of individual meteorites over one another, at the distances involved in the problem, is exceedingly small, while the average velocities of known meteorites is high and their moving force correspondingly high. Estimated from present imperfect data, the average velocity of meteorites is of the order of 20 miles per second or more. This is also about the average order of velocity of stars, as now determined, and hence it may fairly be assumed to be the order of velocity of the average matter of the known universe, and may be taken as the working basis for the problem in question. This gives a prodigious kinetic energy to the matter to be assembled, while the gravitative force between the small masses of dispersed matter is relatively trivial. The individual attractions are all that can be considered until after an assemblage is formed, and it is *the formation* of the assemblage that is here in question.

So far as my studies have gone, almost the only conception that seems to offer a remote possibility of the starting of a swarm of meteorites under these adverse conditions lies in the exceptional case of meteorites moving in nearly parallel directions at nearly the same speed and in courses near one another. In this case the moving forces of the meteorites have the same phase and only antagonize

their mutual attractions to the extent of such small differences as may arise from their slight differences of velocity and direction of motion. Under extremely favorable conditions of this kind, two meteorites might come into mutual gravitative control and revolve about their common center of gravity. Then a third one might join them under like conditions, and so on. The plane of revolution of the third meteorite might chance to correspond with that established by the pair it joined, but more probably it would not. Its direction of revolution might be the same, but more likely either transverse in some degree or opposite. It is extremely unlikely that the planes of revolution of any considerable number of meteorites coming thus together would be identical, or that the directions of their revolutions would all be coincident, and hence opposite and cross-revolutions would doubtless result, with obvions liability to collisions, so that in the end the swarm might perhaps develop into a quasi-gaseous condition, though it might retain a revolutionary organization, in which case it would not fall into the class here under consideration.

It must be noted that the conditions assigned for the starting of the growth of such a swarm are very far from being the usual conditions of adjacent meteorites, and hence the accessions to the group in any given period, if the group were started, must be presumed to be few compared to the whole number of meteorites that would pass through the initiating swarm, for of the meteorites that passed the place of the initiating swarm, all those that had opposite or transverse courses of any appreciable angle and all those that though moving in parallel directions had appreciably different velocities would traverse the swarm with dangerous contingencies. They wonld hence be liable to break up the initiating swarm by colliding with its members and driving them beyond their mutual gravitative control. This contingency is especially great while the swarm is small and its gravitative command of its members feeble. Hence there arises a serious question whether the swarm's peril of destruction is not greater than its chance of growing to a self-protecting size—so incomparably greater, indeed, as to render the method an improbable one. The dangers of infancy in this case seem to be obviously and perilously extreme and the chances of escape exceedingly rare.

A second serious difficulty in organizing hypothetically a swarm of meteorites from discrete matter primitively diffuse was found to lie in the extreme tenuity of the dispersed celestial matter, whether the present amount of such dispersed matter be considered or the

whole of known matter be theoretically dispersed through the space now occupied by it. The light of a star in a flight of fifty years does not encounter enough dark matter to seriously dim its brightness. All the matter that lies between us and the uttermost visible stars does not cut off as much light as a thin cloud. If all the matter now aggregated in the stellar system, on any reasonable estimate of its mass (and the known distribution and movements of the celestial bodies limit such an estimate), were distributed through the space now occupied by the stars, it would not help the case much, so far as the meteoritic assemblage is concerned. To illustrate, if the matter of the solar system were scattered through that portion of space which may be said to be its fair apportionment—that is, the space about it, stretching out half-way to the nearest stars—its tenuity would be such that if the orbit of Neptune were to be regarded as the hoop of a drag-net 5,600,000,000 miles in diameter, and were to be made to sweep through this space at the rate of 12 miles per second—the estimated velocity of the sun—it would take some 900,000,000,000 years for it to sweep up the scattered matter. This is probably not an unfair illustration of the average tenuity of the supposed dispersion, since the sun is apparently near the center of the known system where star-grouping might be expected to be at least as dense as the average of the whole.

With such extreme tenuity of dispersion, even when all known matter is converted into meteorites, and with such potent obstacles to assemblage as are imposed by the high moving force of the meteorites, it seems an imperative conclusion that the growth of a meteoritic assemblage of the mass of the solar system must require a period quite beyond comprehension.

This conclusion led on the further inquiry whether a swarm of meteorites could perpetuate itself *as a swarm* through such a prodigious period. Must not the part first assembled pass on through its own evolution, whatever that might be, without awaiting the excessively delayed assemblage of the later portions? If the members of the swarm were in collisional relations, must not the kinetic energy of the earlier assemblage have been exhausted long before the accession of the latter part? In other words, must not the first assemblage have become solid at a relatively early stage in the process and the remainder of the accessions have been added individually, as meteorites are now added to the sun and planets? Is it a tenable view that the *assemblage* of a swarm should go on alone without attendant evolution until the mass necessary for a solar

system is attained, and then, *but not till then*, enter upon an evolution into a sun-and-planet system? If the swarm was organized on the collisional basis, nothing but a negative answer seems to me possible. If the meteorites could be supposed to so come together as to revolve in harmonious orbits about a common center, on the planetary basis, the assemblage might perhaps be perpetuated; but this takes the case out of the typical meteoritic class, as here defined, and carries it over to the planetesimal.

Under the conditions of the case as thus brought out, I have been unable to discover a probable method by which a meteoric nebula of the quasi-gaseous or collisional type can grow up *de novo* by the assemblage of dispersed meteorites or by the aggregation of chaotic matter if the material were endowed with the present momentum of the average matter of the stellar system.

(2) The study of the possibilities of the origin of a meteoritic nebula of the collisional or quasi-gaseous type from the dispersion of some previous large body recognized three possible phases: (*a*) dispersion by explosion; (*b*) dispersion by collision; and (*c*) dispersion by tidal disruption.

It is difficult to find any tangible ground for postulating an explosion competent to disperse to the requisite degree a body of the mass of the solar system; but if this difficulty be passed and the requisite force be assumed, it must apparently act radially, in the main, and after the matter has made its outward excursion and is arrested by gravitation, it must return on nearly direct lines and collide at the virtual point of departure. If the outward movement were of nebular extent the collision attending the return must have developed sufficient heat for the conversion of the whole into a gaseous body, and the subsequent evolution must have followed gaseous lines. It is not apparent how anything properly analogous to a meteoritic swarm could be developed by this process. If the hypothetical explosion could be supposed to be sufficiently violent to project the constituent matter beyond the control of the system, the dispersed parts might become truly meteoritic, but their courses would be indefinitely divergent, and there would be no assignable agency for their reassemblage. The constituents would pursue individual courses and be subject to sporadic capture essentially as in the case previously considered.

Regarding the possibilities of dispersion by collision, it seemed necessary to suppose that the heat developed would be so great as to convert the main mass into a gaseous state. If the collision were

a center-to-center encounter, a radial dispersion of matter transverse to the line of collision would probably follow, returning from which the material would again collide and, after a series of oscillations, would gradually settle down into a pulsating gaseous mass.* Here again the system would become gaseous at the outset, and probably develop nothing of the typical meteoritic kind, except possibly such sporadic elements as might be projected beyond the control of the system. If the collision were eccentric, a rotatory motion would doubtless be superposed upon the radial motion, and the case would fall under either the gaseous or the orbital system or under a combination of the two.

In the line of my own suggestion † that stellar bodies passing close by one another, but not colliding, may suffer disruption through their differential attractions on one another, aided by internal elasticity, on the principles developed by Roche, Maxwell, and others, I have been unable to find any plausible grounds for postulating a conversion into a meteoritic nebula of the collisional type.

In the case of such a disruption, the scattered constituents must apparently be given a rotatory movement in a common direction and in the orbital plane of the two bodies initiating it. The dynamics of the system are, therefore, from the outset, definitely of a rotatory or revolutionary kind, and the case falls under the orbital or planetesimal system rather than under the meteoritic system.

It appears, therefore, that neither explosion, nor collision, nor tidal disruption is likely to give rise to a distinctively meteoritic swarm of the kind defined, and I have been unable to discover any other source that can be assigned on definite grounds with a workable probability. *Individual* meteorites and rotatory and revolutionary assemblages of dispersed elements, as well as true gaseous nebulæ, may be supposed to arise from the catastrophes named, but apparently these catastrophes are not appropriate agencies for producing fragmental *swarms* of the distinctively meteoritic type.

I have made some study of meteorites to see if their characters have any decisive bearings on the mode of their origin.

Among the distinctive and significant characters of meteorites are their fragmentary forms, their brecciated structures in part, their occasional slickensided surfaces, their veins, the glassy nature of a

*A case of this kind is described by Kelvin, Popular Lectures and Addresses, I, p. 413.

† On the Possible Function of Disruptive Approach in the Formation of Meteorites, Comets, and Nebulæ. Astrophys. Jour., Vol. XIV, 1900, pp. 17–40.

part of their material, the amorphous nature of another part, and the crystalline nature of still a third and larger part, the variations in the coarseness of the crystallization, the extraordinarily large crystals of the nickel-iron, the inclusion of non-metallic crystals and nodules in the nickel-iron crystals, the scattered condition of iron crystals among silicate crystals in many cases (sporadosiderites), the presence of peculiar spheroidal aggregations (chondri), the fragmental nature of these in many instances, the absence of water and hydrates, the absence of free oxygen, the large proportions of the nickel-iron and the magnesia, the absence of a group of minerals common in terrestrial igneous rocks, viz, quartz, orthoclase, the acid plagioclases, the micas, the amphiboles, leucite, and nephelite, the presence of certain unstable chlorides, sulphides, and phosphides unknown in the earth, and the presence of volatile and combustible hydrocarbons.*

These make up a remarkable group of characters, whose origin can spring only from an equally peculiar combination of conditions.

While the fragmental condition of many meteorites on reaching the earth is due to fracturing in their passage through the air, there are indications in many cases that they already had a fragmental form when they entered the atmosphere. This implies that they are portions of larger bodies, and that they were not aggregated, as such, in free space. At least this appears true in the case of most of those more massive ones that reach the surface of the earth. This of itself does not exclude the view that meteorological aggregates may take place in free space, and that these may have entered into the make-up of the larger body from which the meteorites were derived. It, however, bears on the question whether meteorites, as a rule, were organized as such by the gathering together of gaseous matter or scattered particles in open space.

Less equivocal evidence may be found in the fragmental structure of many of the stony meteorites. Among the broken elements are fragments of chondri. As the chondri are aggregations peculiar to meteorites, their fragmentation implies disruption and reassemblage in the parent body, or at least in an antecedent condition. Interest and point are added by the occurrence of larger chondri inclosing fragments of smaller ones. A very singular case of breccia is presented by the Mount Joy meteorite, which is an aggregate of iron

* An excellent sketch of the characteristics of meteorites is given by Dr. O. C. Farrington, Jour. Geol., Vol. IV, 1901, pp. 51, 174, 392.

fragments. These various evidences of fragmentation imply a previous history affected by successive conditions of accretion and fracturing.

The pressure of slickensided surfaces implies a parent body which was subjected to varying stresses, resulting first in fracture and afterward in the rubbing of the fissure walls upon one another. The existence of veins also implies fracture attended by subsequent filling.

The general prevalence, but partial absence, of crystallization and the kinds of crystallization imply varied thermal conditions in the parent body. The amorphous condition implies the absence of fusion and of the conditions of crystallization. The glassy structure equally implies a molten state followed by quick cooling, while the various grades of crystallization imply high temperatures variously sustained. The extremely large crystals suggest protracted high temperature, with conditions favorable for a highly systematic rearrangement of the material. At the same time the frequent cases in which the metallic iron is scattered through the silicate material seem to imply the absence of a completely fluid state, for in that case segregation of the heavy metallic material toward the center of the body should take place. The same is perhaps indicated by the frequent presence of nodules of sulphides and phosphides within the masses of iron. These conditions seem best explained by a prolonged high temperature acting on a mass of mixed material and furnishing conditions suitable for slow aggregation and crystalline rearrangement without complete fluidity being reached.

It is hard to believe that these coarse crystallizations could have been formed in small masses of matter projected into space in the molten condition, and the view that meteorites are formed directly from lavas shot into space by volcanic or other explosive action, as from a sun, a planet, or the moon, is unsatisfactory in this particular. Equally adverse to this view is the extraordinary fact that certain classes of meteorites are formed chiefly of *hydrocarbons* which are volatile at moderately high temperatures and are readily combustible. These hydrocarbons seem prohibitive of high temperatures at all stages of their history, and it is a marvel that they should survive the transit through the atmosphere ; but this is probably due to the fact that they were excessively cold when they entered it and during the brief time of their transit were only superficially consumed, while their interiors remained cold, as the interiors of meteorites are not infrequently found to be immediately after their fall.

Igneous processes on the earth give rise to magmatic differentiation resulting in a familiar series of minerals which make up large portions of the crystalline rocks of the earth's surface; so also weathering and solution remove more of the basic than of the acidic constituents of crystalline rock, and when the residue is metamorphosed a similar series of minerals arises. Among these are quartz, orthoclase, the acid plagioclases, the micas, and the amphiboles—a group absent from the meteorites. This absence suggests that in the parent body magmatic differentiation of this kind and selective weathering did not take place. This, however, does not necessarily exclude volcanic action, nor non-hydrous weathering, but merely the dominant phases of weathering and magmatic differentiation that prevail in the earth and probably in similar bodies having atmospheres and hydrospheres.

The absence of water, of hydrates, and of free oxygen adds its testimony against the derivation of the meteorites from the crusts of all bodies like the earth.

The high velocities and the diverse directions of the meteoritic flights relative to the earth forbid assigning their origin, in general, to volcanic action in the moon or in any of the planets. Sufficient velocity might be given by a solar explosion, but the directions would be radial and not promiscuous. Explosive action from the members of the solar system may have made an occasional meteoroidal contribution, but scarcely more than that.

Taken altogether, the combination of characteristics presented by meteorites seems to fail of satisfactory explanation in any hypothesis of their direct derivation from a sun or star, or from a planet surrounded by a hydrosphere or an oxygen-bearing atmosphere, or from any planetary body affected by mineralogic differentiations of the terrestrial type. No more do they seem to find satisfactory explanation in simple accretion in free space.

It remained to inquire whether small atmosphereless bodies like the asteroids and the satellites afford a more probable source. Following the doctrine of Stoney, small celestial bodies are believed to be devoid of atmospheres and hydrospheres because their gravity is too low to overmatch the molecular velocities of the atmospheric gases and the vapor of water. This interpretation carries the corollary that they never have had permanent atmospheres and hydrospheres. They thus meet the criterion imposed by the absence of oxygen and water. If built up by accretion, they should contain the requisite variety of material, and if formed in some other way

they may have had it. In their different parts they may present the required structural characteristics. I see no reason to doubt that the asteroids and satellites have been subjected to deformations attended by fractures, brecciation, veins, slickensides, and similar dynamic phenomena. Eruptive and explosive action as well as the impact of falling bodies from the exterior may have contributed various forms of fragmental and amorphous material. The absence of a protecting atmosphere subjects their surfaces to the full striking force of falling bodies, and also the disrupting effects of extreme changes of temperature. On the exterior, amorphous masses, as well as glassy and cryptocrystalline rock, may not improbably be formed, while at greater depths the varying conditions of pressure and temperature requisite for the more complete and coarser crystallizations may probably be present. The hydrocarbons may be assigned to inorganic action within the asteroidal body, the material being derived from the hydrogen and carbon gases so abundantly occluded in meteorites and crystalline rocks, the requisite temperatures and pressures being supplied by the internal compression of the body.

In these small bodies, then, it is perhaps possible to find that extraordinary combination of conditions which the nature of the meteorites implies.

It remains to postulate a means of disruption and dispersion by which the disrupted fragments shall be given the erratic courses and the high velocities which meteorites possess, while at the same time the structural features, sometimes rather perishable, shall escape destruction by liquefaction or extreme pulverization.

Any supposed explosion from an internal source is unsatisfactory, because it is difficult to assign a probable and sufficient cause for an explosion capable of imparting a velocity of several miles per second, which would probably be required to disperse the fragments beyond the control of the system to which the body belonged, and because if such sufficient explosion were realized, it must apparently wreck many of the peculiar meteoritic structures.

Collision with some other body at a high velocity would be sufficient to disrupt the body and drive its fragments away with the requisite velocity, but the imminent danger of liquefaction by the inevitable heat of the impact or of extreme pulverization of the fragile material raises doubt as to the adaptability of collision to give origin to the hydrocarbon and some of the stony meteorites of large size, while it might well give rise to minute meteorites. The

relative rarity of collision also suggests that it should be assigned a secondary place.

It has been suggested* recently that disruption by differential attraction might satisfy the requirements of the case, though there is perhaps some ground for doubt as to its adequate frequency. According to principles established by Roche, Maxwell, and others, a small body passing within a certain distance (the Roche limit) of a larger dense body will be torn into fragments by differential attraction. The size of this sphere of disruption depends on the densities, cohesion, internal elasticities, and other factors of the two bodies. For incompressible fluids of the same density Roche gives the limit of disruption as 2.44 times the radius of the large body. In most such bodies internal elasticity probably exceeds cohesion, and the sphere of disruption would be larger than this. The moon would probably expand with some violence if its gravity were suddenly removed by differential attraction. In any case fragmentation in this way would be several times more probable than an actual collision. Furthermore, the fragmentation in this case is not minute nor violent, and this fits the meteoritic requirements.

Relative to their erratic courses, it may be noted that a small body passing near a much larger body is liable to be thrown from its previous orbit into quite a new one. As is well known, this has apparently happened to several comets through the influence of the planet Jupiter. As shown by H. A. Newton, if the orbit of the small body is such that it is caused to pass close in the rear of the large body, say the planet Jupiter, its course will be diverted into a larger orbit. If a small body were to pass in this way sufficiently near to Jupiter, it would be thrown entirely out of the solar system, and its path thence would probably be as unrelated to any stellar system as that of an average meteorite.

In these two sets of principles there is a combination peculiarly fitted for the results required, for by their joint action a small body passing near a large body is liable to be disrupted into fragments, and these at the same time to be thrown into erratic courses, which may carry them entirely outside the system to which they belonged and give them independent courses in stellar space. It is obvious that fragmentation and dispersal by the differential attraction of very close approach escapes the adverse contingencies of liquefaction and pulverization incident to explosion or collision.

---

* On the Possible Function of Disruptive Approach in the Formation of Meteorites, Comets, and Nebulæ. Jour. Geol., Vol. IX, 1901, p. 369.

If the question be pushed a step farther, to inquire how small bodies like the asteroids may be rendered specially subject to the requisite conjunctions, the answer may be found in the approach of suns to one another, attended by such secondaries. For example, if the solar system were to pass even within five or six billion miles of a similar system, the orbits of the secondaries would be very greatly perturbed and an intricate and prolonged series of changes would ensue. These are too complicated to be followed by computation, but there are grounds for believing that they might involve, sooner or later, through their disturbed courses, the close approach of some of the smaller bodies to some of the larger. These smaller bodies in the solar system are numbered by hundreds, and similar numbers may be suspected to belong to other systems, and this largeness of number adds to the probabilities of some close approaches during a condition of general disturbance.

The solar system is probably not the most favorable selection for illustrating the contingencies of such disturbance, for it is a simple isolated system, with a single overpowering center that convoys its attendants by a scarcely disputed control. From its symmetry, it is to be inferred that it has swept through space undisturbed throughout the period of its existing organization. But there are many binary, triple, multiple, and clustered systems of suns which apparently divide the control of a common field, and this divided control may reasonably be supposed to involve approaches of the chief bodies of sufficient nearness to one another to perturb seriously their outlying secondaries and introduce disturbances ultimately involving disruptive approaches. The nebulous matter associated with some of these perhaps implies something of this kind.

The hypothesis of disruption by differential attraction may be pushed one step farther by postulating that the disrupted group of fragments may in its earlier history constitute a comet, since it is the general belief of astronomers that the comet's head is composed of a cluster of small bodies. The peculiar emanations which arise from a comet may perhaps as plausibly be referred to the occluded vapors and the radio-active substances of a shattered asteroid as to any other recognizable source. The recent discoveries of the prevalence of radio-activity and allied phenomena render the cometic emanations less strange and exceptional than they once seemed.

The fragments of an asteroid or other small body disrupted in this manner would, it is believed, be given a rotatory movement by the differential attraction that produced them, and hence the result-

ing cluster of fragments should revolve about their common center of gravity in a somewhat definite plane, but at the same time in more or less irregular and inharmonious paths, as the result of the incidents of disruption, and these doubtless render them subject to mutual disturbance and frictional and glancing collisions.

It is now accepted as highly probable that comets, particularly those that have short orbits and frequently return to the vicinity of the sun, are gradually dispersed by the latter's differential attraction. The mutual gravity of the cometic fragments being very small, the differential gravity of the sun in its own neighborhood becomes superior to it, and the members of the cometary cluster are drawn apart, and thenceforth revolve about the sun in their own individual orbits, irrespective of the other members. In other words, the cluster of fragments that is supposed to constitute the comet's head passes into *the planetesimal state* by dispersion. In this we seem to have an actual instance of that tendency of a swarm to pass into a planetesimal condition to which allusion has heretofore been made.

These planetesimals constitute one variety of meteoroidal bodies in the broader sense of the term meteoroidal, and it is to these that the brilliant August and September meteoric showers are assigned. It has not been quite demonstrated that they are identical with the iron and stony meteorites above described, for they do not generally reach the earth, and it is not positively known that they have done so in any case, but their essential identity is extremely probable. In the fact that they have come to have individual orbits about the sun, and that these orbits are parallel to one another, and that their velocities are of the same order, they do not represent the typical meteoritic condition as heretofore defined. They illustrate rather the planetesimal mode of organization.

The foregoing hypothesis of the origin of meteorites makes them but an incidental result of stellar and planetary action. If this be correct, their genesis is wholly a secondary matter, and furnishes no ground for regarding meteorites as the parent material of great nebulæ or of stellar systems. The quantity of matter dispersed in this way is, by the terms of the hypothesis, limited to an extremely small part of the total mass of the systems from which it is derived. This scattered matter is presumed to be picked up individually by all the larger bodies, as is being done daily by the earth, and the maintenance of the supply only requires the disruption of small bodies to an extent equal to the trivial masses gathered in by the existing suns

and planets. The exceedingly small amount of meteoritic material picked up by the earth seems to be consistent with this interpretation.

In conclusion, it may be remarked that, so far as my studies have gone, the meteoritic condition seems most probably to be an incidental result of cosmic mechanics of trivial importance, and to be a source of merely incidental accretion to existing bodies. Meteoritic aggregation of the type defined does not seem to represent a great generative method whereby stellar systems are evolved. On the contrary, the meteoritic condition seems to be inherently moribund, passing into the gaseous state on the one hand, or into the planetesimal on the other, or, in the absence of assemblage, losing its constituents to existing suns and planets by capture one by one.

A much larger portion of my study during the past year has been devoted to a development of the planetesimal hypothesis into greater precision and detail, to the applying of such tests as I could devise, and to the working out of its concrete relations to the many geological problems whose solution is vitally dependent on the mode of the earth's origin. From the geological point of view the ultimate test of this hypothesis and of all other hypotheses of the earth's origin lies in their working qualities. As a complete statement of the planetesimal hypothesis has not yet appeared in print, it will doubtless be best that I should outline with some detail the form the hypothesis has assumed as the result of the work upon it, particularly as this will best indicate the work that has been done.

Under the typical form of the planetesimal hypothesis it is assumed that the parent nebula of the solar system consisted of innumerable small bodies, planetesimals, revolving about a central gaseous mass, somewhat as do the planets to-day. The hypothesis, therefore, postulates no fundamental change in the system of dynamics after the nebula was once formed, but only an assemblage of the scattered material. The state of dispersion of the material at the outset and throughout, as now, was maintained *by orbital revolution*, or, more closely speaking, by the tangential component of the energy of revolution. The planetesimal hypothesis by no means excludes gases from playing a part in the parent nebula or in its evolution, any more than it denies their presence in the sun or the atmosphere to-day, but it assigns to gaseous action a subordinate place in the evolution of the planetary system after the planetesimal condition had become established.

An inquiry into the possible modes by which the planetesimal

condition might arise revealed several possible methods. Such condition might arise from a nebula that was originally gaseous. If, for example, it be supposed that the parent nebula was a gaseous spheroid, and that it detached material from its equatorial belt molecule by molecule, rather than by rings, as postulated by Laplace, these molecules would probably become planetesimals instead of members of a true gaseous body. It is not the thought that these molecules would be thrown off directly into planetesimal orbits, because their initial paths would probably be ellipses that would bring them back to the point of departure; but that, by certain classes of collisions while in these elliptical orbits, they would be diverted into orbits that would not bring them again into collision with the parent spheroid. There is reason to believe that this method would really be almost the only systematic one by which a gaseous spheroid of the Laplacian type would detach material from its equatorial belt.

But if this be not true, and if an earth-moon gaseous ring were formed, as assumed in the Laplacian hypothesis, computation shows that its attractive power at any one point on its surface would be very low. If the present earth were converted into a solid ring, occupying its present orbit, it would have a diameter of about 25 miles with its present average density. Computation is scarcely necessary to show that the gravity of this ring at any point on its surface would be very feeble, and it is obvious that this gravity must be greater than the gravity on the surface of the same matter if it were dispersed by intense heat into the form of a gaseous ring. The application of the kinetic theory of gases to such a ring, under the postulated temperature, forces the conviction that the molecules would have been so driven apart by mutual collision and rebound that they would have become essentially independent of one another, each revolving in its individual orbit, with only rare and incidental collisions. In other words, they would have become planetesimals controlled by the central mass and not a gaseous aggregate controlled by its own gravity. They would, therefore, not have been concentrated by direct attraction on the principles controlling a cooling gaseous body, but would have been subject to accretion one by one in the modes presently to be described.

Under certain circumstances meteorites might be assembled in such a way that they would come to revolve in concentric orbits about their common center of gravity, as previously indicated, and thus assume a quasi-planetesimal condition in contradistinction to that of a quasi-gaseous swarm of meteorites, in which each is habitually

drawn toward the center, collides, and rebounds after the fashion of gaseous molecules, as conceived by Lockyer and Darwin. The meteoroids that are formed by the dispersion of a comet, such as constitute the belts that give rise to the August and November meteoritic showers, are probably in the planetesimal rather than the collision-rebound condition, and are becoming more and more scattered and individually independent as time goes on.

As the basis for developing the typical form of the planetesimal hypothesis, I have assumed that the parent nebula had a planetesimal organization from the outset. The conception is a rather radical departure from the gaseous conception of the familiar nebular hypothesis, and from the meteoritic conception of Lockyer and Darwin, so far as fundamental dynamics and mode of evolution are concerned. To develop the hypothesis as definitely and concretely as possible, I have further chosen a special case from among those that might possibly arise, viz, the case in which the nebula is supposed to have arisen from the dispersion of a sun as a result of close approach to another large body. The case does not involve the origin of a star nor even the primary origin of the solar system, but rather its rejuvenation and the origin of a new family of planets. The general planetesimal doctrine does not stand or fall with the merits or demerits of this special phase of it, but to be of much real service in stimulating and guiding investigation, a hypothesis must be carried out into working detail so that it may be tested by its concrete and specific application to the phenomena involved, and hence the reason for developing a specific sub-hypothesis. This particular sub-hypothesis was selected for first development (1) because it postulates as simple an event as it seems possible to assign as the source of so great results, (2) because that event seems very likely to have happened, (3) because the form of the nebula supposed to arise in this way is the most common form known, the spiral, and (4) because spectroscopic observations seem at present to support the constitution assigned this class of nebulæ, although it must be noted that spectroscopic observations have not reached such a stage of development as to demonstrate the motions of the nebular constituents. In future spectroscopic determinations lies one of the crucial tests which the hypothesis must yet undergo, for there is little doubt that spectroscopic work will in time reach such a degree of refinement as to demonstrate the motions of the constituents of the spiral nebulæ.

Present spectroscopic data relative to the constitution of the nebulæ, considered in relation to the question in hand, reveal two general classes of nebulæ, the one characterized by bright spectral lines, the other by a continuous spectrum.

The first are usually said to be gaseous, but this designation is not sufficiently accurate for our present purpose. The bright lines of the spectrum can only be affirmed to indicate that the matter of these nebulæ is in a *free-molecular* condition. They do not certainly indicate whether (1) the molecules are in the collisional relations of gaseous molecules or (2) are scattered widely, like meteorites, so that collisions are rare and incidental, or (3) are moving on radiant or on parallel lines, or (4) are pursuing concentric orbits, and are thus planetesimal in dynamic character. For the purposes of this study, where dynamic distinctions are important, these nebulæ may be designated, with due reserve, simply as *free-molecular nebulæ*. They often have a greenish cast from the predominance of green lines in their spectra, and are conveniently styled green nebulæ. The bright spectral lines indicate the dominance of hydrogen, helium, and an otherwise unknown element or elements, provisionally called nebulium. There are occasionally a few other non-metallic elements, but *metals have not been detected*. Their constitution, as now determined, does not, therefore, fit them for the parentage of our earth, in which metals abound and in which hydrogen and helium are subordinate elements, while nebulium is unknown. The possibilities of transmutation into suitable elements can not, to be sure, be safely denied in these days of revolutionary discoveries, but, on the other hand, can not very safely be made a working basis. The class includes the "planetary," the "stellar," the "ring," and most of the irregular nebulæ.

Almost identical with the spectra of these nebulæ are the spectra developed in an early phase of the declining stages of the new stars that occasionally flash forth with sudden brilliancy and soon die away to obscurity or extinction, continuous spectra sometimes marking the later stages. While the origin of these "*Novæ*" is unknown, the conjecture that they are due to collision or to explosion has been entertained, and this conception has also been extended to the free-molecular class of nebulæ. It is a further suggestive fact that these early spectra of the new stars and the spectra of green nebulæ are closely similar to the spectra of the "helium stars" and the "hydrogen stars," which astronomers usually place in the first or "earliest" group in evolutionary classifications of

the stars. There is thus much of ground, therefore, for linking together in genetic studies these stars, the *Novæ* and the helium-hydrogen-nebulium nebulæ and for looking upon them provisionally as primitive states. If our quest were the *genesis of stars*, these would seem to point the way, so far as anything does at present; but our quest is the genesis of the solar *family of planets*, in one of which our study centers, and the genesis of our earth is not necessarily and immediately connected with the genesis of stars. Nebulous bodies composed of helium, hydrogen, and the hypothetical nebulium might, for aught we dare now affirm, remotely evolve into material of the complex terrestrial type; but the speculation is rather too bold for prudent use as a basal factor in a conservative hypothesis.

*The forms* of the helium-hydrogen-nebulium nebulæ are scarcely more promising for planetary evolution when their dynamical properties are considered. While observation has as yet determined almost nothing as to their internal movements, their forms do not encourage the belief that they would under known laws evolve into a system characterized by the peculiar distribution of mass and momentum which the solar system presents. For the present, therefore, these nebulæ have been passed by in the search for the immediate genesis of the earth.

The other class of nebulæ give continuous spectra and are conveniently styled white nebulæ. The continuous spectrum is interpreted to mean that their chief luminous material is in a liquid or solid state, or, perhaps better, that the molecules are in an *aggregated* state, in distinction from the free state of the previous class. As the liquid condition is limited to a rather narrow range of temperature, and as this range is very different for different material, it is improbable that any large portion of a nebula is in this state, and the whole may be conveniently treated as though it were formed of solid matter, but matter in a finely divided condition. This last qualification seems necessary, for the volume of these nebulæ is often very great, and yet they appear to intercept but little light and give no signs of great attractive power.

The prevailing form of these nebulæ is the *spiral*, as determined by the late Professor Keeler, and this form particularly characterizes the smaller nebulæ recently brought to knowledge by improved instruments and manipulative skill. These newly discovered nebulæ are estimated to number at least ten times the whole number previ-

ously known. From the superior number of spiral nebulæ it is a safe inference that their peculiar forms represent some prevalent process in celestial dynamics. This is in itself a reason why research should turn to them, by preference, for the origin of the present solar system.*

Nothing is yet positively known of *the motions* of the parts of these spirals, for time enough has not yet elapsed since they were first sharply photographed to permit the requisite comparisons. Inferences from their remarkable forms are the only present resource. To me these peculiar forms seem to imply that the spirals sprang from *a combined outward and rotatory movement*. The outward movement may no longer exist, as it may have been already checked by the gravity of the central mass, and the rotatory motion be the dominant one at present, but their forms seem still to bear the impress of an outward movement. If the outward movement has ceased, or when it ceases, the rotatory movement must tend to wrap the spiral up more and more closely and symmetrically, because the revolutions of the inner parts must be more rapid than those of the outer parts. By this it is not meant that the matter of the nebulæ is necessarily drawn nearer the center of the system, but merely that the arms are stretched and more closely coiled. The forms that seem to be the more mature appear to betray this, for their inner parts are coiled more closely and symmetrically than their outer parts. In the

---

* The profoundly lamented death of Professor Keeler, just as he was beginning to reap the rich fruits of his skill and patience in nebular investigations, gives historical value to his latest statement of results, published about two months before his death.

"1. Many thousands of unrecorded nebulæ exist in the sky. A conservative estimate places the number within reach of the Crossley reflector at about 120,000. The number of nebulæ in our catalogues is but a small fraction of this.

"2. These nebulæ exhibit all gradations of apparent size from the great nebula in *Andromeda* down to an object which is hardly distinguishable from a faint star disk.

"3. Most of these nebulæ have a spiral structure. * * *

"While I must leave to others an estimate of the importance of these conclusions, it seems to me that they have a very direct bearing on many, if not all, questions concerning the cosmogony. If, for example, the spiral is the form normally assumed by a contracting nebulous mass, the idea at once suggests itself that the solar system has been evolved from a spiral nebula, while the photographs show that the spiral nebula is not, as a rule, characterized by the simplicity attributed to the contracting mass in the nebular hypothesis. This is a question which has already been taken up by Professor Chamberlin and Mr. Moulton of the University of Chicago." Astrophys. Jour., June, 1900, pp.347–348.

remarkable nebula in Canes Venatici there are curved streamers, like the tails of comets, stretching outward from some of the knots in the arms. If these are indeed streamers driven outward from the knots and curved by motion, as in the case of comets' tails, they testify very definitely to a rotatory movement.

A notable and seemingly very significant feature of these nebulæ is the presence of *two dominant arms* that arise from diametrically opposite sides of the nucleus and curve concentrically away. No single-arm spiral of the watchspring type has been found, so far as I am aware. There are often more than two arms in the outer part, and there is much irregularly dispersed matter, but even in the more scattered forms the dominance of two arms is discernible.

A second feature of note is the presence of numerous *nebulous knots* or partial concentrations on the arms and more or less outside them. So, also, the more diffuse nebulous matter is unequally distributed, and in some of the forms, regarded as youngest, dark spots and lines emphasize the irregularity.

All these features go to show that these forms are controlled, not by the support of part on part, as in a continuous body or in a mass of gas or even in a definite swarm of quasi-gaseous meteorites, but by some system of combined kinetic energy and gravity which *permits independence of parts.* It is, therefore, conceived that the innumerable solid or liquid particles which the continuous spectrum implies revolve about the common center of gravity as though they were planetoidal bodies. If this were certainly known to be the case, these might well be called *planetesimal nebulæ.*

It is clear from the tenuity of these nebulæ, as seen from the side of the spiral, that they are disk-like, and this is directly shown to be so when they are seen obliquely. In their disk-like shape, these nebula conform to the mode of distribution of matter in the solar system. Within the area of their disks also, the distribution is irregular, as it is in the solar system—a fact too much overlooked by reason of our predilection for symmetry, under the influence of the symmetrical Laplacian conception.

All of the more familiar spiral nebulæ have dimensions that vastly transcend those of the solar system, and they can not be taken as precise examples of the solar evolution. Because of these vast dimensions and of the probable feebleness of control of the central mass, which often appears to be itself quite tenuous, a rapid motion can not well be assigned to the arms. Seen from the immense

distances at which these nebulæ seem to be placed—no parallax having been as yet detected—changes of position must necessarily be slow in revealing themselves to observation. It is to be hoped, however, that the present rapid progress in the perfection of instruments and of skill will soon bring within the reach of successful study some of the smaller spiral nebulæ that represent the solar system more nearly in mass and proportions.

With this much of knowledge and of limitation of knowledge relative to existing nebulæ, the construction of a working hypothesis required not a little resort to supplementary deductive and hypothetical considerations. The inference that a spiral nebula is formed by a combined outward and rotatory movement implies a preëxisting body that embraced the whole mass. In harmony with this, an ancestral solar system has been postulated—a system perhaps in no very essential respect different from the present one. My hypothesis does not, therefore, concern itself with the primary origin of the sun or of the stars, or with the ulterior questions of cosmic evolution. It confines itself to a supposed episode of the sun's history in which the present family of planets had its origin, and in the initiation of which a possible previous family may have been dispersed, but no affirmation is made relative to this. With some partiality, perhaps, this episode may be regarded as geologic, since it specially concerns the birth of the planet of which alone we have intimate knowledge.

To this conception of an ancestral sun with an undefined antecedent history as a star, question will arise at once as to a sufficiency of energy for the sun's maintenance through such a prolonged history. It has been strongly urged during the past half-century by very eminent physicists that the resources of energy assignable for the maintenance of the sun's heat and light could, at best, be barely sufficient for the geological and biological demands of the earth's known history, even when these are most conservatively estimated; how much less then can they be sufficient for an antecedent history of unknown duration. This objection is based on the assumption that the sun's heat and light are derived *almost wholly* from self-compression, as urged by Helmholtz. This self-compression has usually been computed on the basis of certain limiting assumptions, the validity of which is open to question.

That self-compression is a potent source of heat is not doubted, but the Helmholtzian theory takes no account of sub-molecular and sub-atomatic sources of energy. The transcendent potency of these

sources of energy has been some time suspected,* and is now being revealed by refined physical research. The extraordinary energies displayed by radio-active substances are doubtless but an initial demonstration of immeasurable energies resident in other forms of matter and in the constitution of the sidereal system and competent for its maintenance for unassignable periods. It does not appear, therefore, in the light of recent revelations in physics or recent discoveries in the constitution of the stars and the stellar systems, that there is any sufficient reason for setting narow limits to the life of the sun. It seems more in accord with recent advances in knowledge to place the compressional theory of the sun's heat in the category of the earlier chemical and meteoritic theories as true and contributory, but as only partial and inadequate.

There seem to be no sufficient grounds, therefore, for hesitating to postulate an ancestral solar system, the center of which was the parent of the present sun. This involves the further quite reasonable assumption that the sidereal system has had a very prolonged history, and that the ancestral sun played its own part in it as the solar system does now.

* I wrote in 1899, before experimental demonstration had been reached: "Without questioning its correctness, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a complete theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly no careful chemist would affirm either that the atoms are really elementary, or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position, and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. The Helmholtzian theory takes no cognizance of latent and occluded energies of an atomic or ultra-atomic nature. A ton of ice and a ton of water at a like distance from the center of the system are accounted equivalents, though they differ notably in the total sum of their energies. The familiar latent and chemical energies are, to be sure, negligible quantities compared with the enormous resources that reside in gravitation. But is it quite safe to assume that this is true of the unknown energies wrapped up in the internal constitution of the atoms? Are we quite sure we have yet probed the bottom of the sources of energy and are able to measure even roughly its sum total?" (On Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life, Science, vol. IX, June 30, and vol. X, July 7, 1899.)

With 100,000,000 or more known suns and an unknown number of dark bodies moving in various directions with various velocities, the possibility of collision is well recognized; but, owing to the vastness of the intervening spaces, the contingencies of collision for an individual sun are small. However, no appeal is here made to collisions as a source of the parent nebula of the solar system, but only to an approach of the ancestral sun to another large body, and this approach is not assumed to have been very close. This rather distant approach is a contingency that may fairly be assumed as likely to have been realized in fact. I have elsewhere discussed the general effects of the close approach of celestial bodies* to one another, but the particular case of the supposed ancestral sun requires special consideration.

Our present sun shoots out protuberances to heights of many thousands of miles, at velocities ranging up to 300 miles per second and more. If it were not for the retarding influence of the immense solar atmosphere, some of these outshoots would doubtless project portions of themselves to the outer limits of the present system, and perhaps in some cases quite beyond it, for the observed velocities sometimes closely approach the controlling limit of the sun's gravity, if they do not actually reach it. The expansive potency of this prodigious elasticity is held in restraint by the equally prodigious power of the sun's gravity. If with these potent forces thus nearly balanced the sun closely approaches another sun or body of like magnitude—suppose one several times the mass of the sun, since it is regarded as a small star—the gravity which restrains this enormous elastic power will be *relieved along the line of mutual attraction*, on the principle made familiar in the tides. At the same time the pressure transverse to this line of relief is increased. Such localized relief and intensification of pressure must, it is believed, result in protuberances of exceptional mass and high velocity. According to the well-known tidal principle, these exceptional protuberances would rise from opposite sides, and herein lies the assigned explanation of the prevalence of two diametrically opposite arms in the spiral nebulæ.

Nothing remotely approaching a general dispersion of the ancestral sun seems to be required. The present planets and their satellites

* On the Possible Function of Disruptive Approach in the Formation of Meteorites, Comets, and Nebulæ. Astrophys. Jour., vol. XIV, No. 1, July, 1901, pp. 17–40; Jour. Geol., vol. IX, No. 5, July–Aug., 1901, pp. 369–393.

altogether amount to about one seven-hundredth part of the mass of the system. Simply to supply the required planetary matter, the protuberances need include but this small fraction of the ancestral sun. However, some considerable part of the projected matter must probably have been gathered back into the sun, and some part may possibly have been projected beyond the control of the system. Making allowances for both these factors, the proportion of the sun's mass necessarily involved in the protuberances is still very small. Apparently 1 or 2 per cent of the sun's mass would amply suffice.

The protuberances, by hypothesis, would be thrust out as the sun was swinging about its temporary companion star in a sharp curve, and necessarily at a prodigious velocity. It is inferred that the projected protuberances would be differentially affected by the attraction of the companion star, and take different curves about it, out of which must spring rotatory motion. This seems logically clear, but the precise paths pursued by the parts of the protuberances would apparently vary widely with different cases. As each case constitutes an involved example of the problem of three bodies, the whole is beyond rigorous mathematical treatment, but special solutions seem to justify the inference that effective rotation would arise.

The distal portions of the protuberances would obviously be formed from the superficial portions of the sun, while the later portions of the ejections forming the proximal parts of the arms would doubtless come mainly from lower depths, and hence probably contain more molecules of high specific gravity. In this seems to lie a better basis for explaining the extraordinary lightness of the outer planets and the high specific gravities of the inner ones, than in the separation, from the extreme equatorial surface of a gaseous spheroid, of successive rings whose total mass only equaled one seven-hundredth part of the original nebula.

It seems consistent with the conditions of the case to assume that the protuberances would consist of a succession of more or less irregular outbursts, as the ancestral sun in its swift whirl around the controlling star was more and more affected by the latter's differential attraction; and hence the protuberances would be directed in somewhat changing courses, and would be pulsatory in character, resulting in rather irregular and somewhat divided arms, and in a knotty distribution of the ejected matter along the arms. These knots must probably be more or less rotatory from inequalities of projection.

It is thus conceived that a spiral nebula, having two dominant arms, opposite one another, each knotty from irregular pulsations, and rotatory, the knots probably also rotatory, and attended by subordinate knots and whirls, together with a general scattering of the larger part of the mass in irregular nebulous form, would arise from the simple event of a disruptive approach.

The ejected matter, at the outset, must have been in the free molecular state, since by the terms of the hypothesis it arose from a gaseous body; but the vast dispersion and the enormous surface exposed to radiation doubtless quickly reduced the more refractory portions to the liquid and solid state, attended by some degree of aggregation into small accretions; hence the continuous spectrum which this class of nebulæ present.

The problem of the luminescence of nebulæ is confessedly a puzzling one. There is little ground for assigning general incandescence to matter so obviously scattered and tenuous and possessed of such an enormous radiating surface. The assignment of the light to the collision of meteorites, as done by Lockyer, encounters both dynamic and spectroscopic difficulties. The recent discoveries of the luminescent properties of radio-active matter and of its power to awaken luminescence in other matter offers some hope of a solution. The fact that these properties are not necessarily dependent on heat greatly relieves the stress of the problem. Whatever of radio-active material there might be in the matter dispersed into nebulous form would by such dispersion be set free for action, and whatever other matter was subject to its excitation would also be set free to receive the exciting influence.

The solution of the problem may, however, lie along electrical lines. At present it seems more probable that the luminescence arises from some agency that acts at low temperatures, than that it is dependent on heat, and hence objections to a planetesimal organization on the ground of low temperature do not seem to me to have much force.

As previously remarked, the verity of this particular mode of origin of spiral nebulæ is not essential to the acceptance of the planetesimal hypothesis. It is merely necessary that two simple assumptions should hold good, viz, (1) that the nebular matter of the spiral be in a finely divided solid or liquid condition, as the continuous spectrum implies, and (2) that the particles of this scattered material revolve in elliptical orbits about the central mass.

In attempting to follow the probable evolution of such a spiral

nebula, three elements stand out conspicuously: (1) The central mass, obviously to become the sun; (2) the knots on the arms that are assumed to be the nuclei of the future planets and perhaps satellites; and (3) the diffuse nebulous matter to be added to the nuclei as material of growth. In the particular case of the solar nebula it is assumed (1) that the central mass was relatively very great; (2) that the knots were very irregular in size and placed at irregular distances from the center; and (3) that the nebulous portion was very small relative to the central mass and probably large relative to the knots.

It is assumed that the masses of matter in the knots were sufficiently large to hold themselves together in spite of the differential attraction of the central mass, otherwise they would soon have been scattered. They seem to have maintained themselves successfully in existing nebulæ that appear to have undergone some notable degree of evolution.

On the other hand, it is presumed that the mutual attraction of the more tenuous nebular matter was insufficient to aggregate itself directly in the presence of the central attraction, for in the existing nebulæ this portion seems to show a progressive tendency to a more general diffusion. The planetesimals of the diffused nebulous portion are assumed to be controlled essentially by the gravitation of the main mass and to revolve in individual orbits about it.

The irregularity of the forms of the knots seems to imply that their organization is also planetesimal, though the larger ones may be able to hold gases also. The direction of revolution of these knots is supposed to be usually the same as that of the rotation of the nebula as a whole, but subject to local and special influences that might lead to important variations.

While the knots of the solar nebula are regarded as the nuclei about which gathered the planetesimals to form the future planets, all such nuclei did not necessarily retain their independence and grow to planets, though no planet probably developed except from such a nucleus. Existing nebulæ show clusters of knots and aggregates of irregular form susceptible of development into complex planetary systems, such as the large planets and their families of satellites. The earth-moon system is assigned to a couplet of companion nuclei of quite unequal sizes.

Certain studies were made to determine the probable amount of growth of the planets, as this possesses much geological interest. Two considerations bear upon the size of the original nuclei.

1. There is a certain necessary limitation to the size of tenuous bodies in the presence of more massive bodies. The principle involved is one of vital importance in the study of planetary evolution. Within the field of the effective attraction of a dominant body like the sun, or the ancestral nebular center, small bodies exercise differential gravitative control over a limited sphere only, known technically as the "sphere of activity." This sphere for the earth, with its *present mass*, reaches out about 620,000 miles.* If the earth has grown at all its primitive sphere of control must have been smaller than this. The earth nucleus could, therefore, only have embraced such matter as lay within this limited sphere. If the original knot could be supposed to have extended beyond this limit the outlying portion would have been drawn away by the solar mass into independent planetesimals, and must have been gathered in, if it became a part of the earth at all, by some other means than direct attraction. The moon controls, as against the attraction of the earth at its present distance, a sphere whose radius is about 25,000 miles, and considerably less than 25,000 miles as against the joint attraction of the earth and sun. Its primitive nucleus, if it has grown at all, was confined to smaller dimensions. Attenuated nuclei of indefinite size can not, therefore, be supposed to maintain themselves permanently in the fields of attraction dominated by larger bodies. Bodies of gas, subject to the dispersive effects of their own molecular velocities, in addition to the competitive attractions of the dominant bodies, have still narrower limits, and, below a certain mass, are inevitably dispersed. In such a system as ours gases must, for the most part, either join themselves to the dominant bodies or be scattered into molecular planetesimals. None of the smaller knots of the solar nebula could probably have been gaseous in any large measure. Gases were probably attached to and occluded in the aggregated or solid planetesimals, and may have been held in a free gaseous state in the interiors of the larger nuclei. The sun is, of course, presumed to have been gaseous throughout the evolution.

2. Quite a definite indication of the size of the nuclei of the planets may perhaps be deduced from the very remarkable fact that Phobos, the inner satellite of Mars, revolves around the planet in *less than one-third* of the time of the planet's rotation, and from the analogous fact that the little bodies which make up the inner part of the inner

* "The Spheres of Activity of the Planets," by F. R. Moulton, Pop. Astron., No. 66, p. 4.

ring of Saturn revolve about that planet in a little more than *one-half* the time of the planet's rotation.* These are exceedingly troublesome facts from the viewpoint of the Laplacian hypothesis, for under it the contraction of these planets, after they had shed their secondaries, should have greatly accelerated their rotations, and these should have become much shorter than the revolutions of the secondaries. Before Moulton's citation of the second case an attempt was made to explain the case of Phobos by a supposed tidal retardation of the planet's rotation, but the Saturnian case appears to render this explanation incompetent.†

Under the hypothesis of growth from a nucleus by the addition of planetesimals, the rotations of the planets were dependent largely on the special phases of the impacts of the infalling planetesimals, and no necessary relations between the rotation of a planet and the revolution of its satellite are assignable. But if this be neglected, and the rotation-period of the planetary nucleus be assumed to have been originally the same as the revolution-period of the satellite's nucleus, the growth of the mass of the planet must have drawn the satellite nearer to itself and shortened the time of its revolution. If the whole of the periodic difference between Mars and Phobos be due to this cause, the growth of the nucleus of Mars is deducible from it. Under this view the matter of the rings of Saturn may have been satellite nuclei at the outset, and have been drawn within the Roche limit by the growth of Saturn, and then disintegrated by tidal action and distributed into the ring form. All other satellites should, under this view, have been drawn toward their primaries during the growth of the latter, and this may be a not unimportant factor in their evolutionary history.

The concurrent bearings of these two considerations are quite in harmony with what might be gathered independently from a comparison of the apparent amounts of matter in the nebular knots with the amounts in the nebular haze in existing nebulæ. It was therefore assumed in my further study that the nuclei constituted only a small portion of the mass of the grown planets. The fraction was probably larger proportionally for the small planets than for the large planets, for the power of growth probably rose with increased mass in geometrical ratio. In the case of the asteroids and satellites the growth may not have been large.

---

* For a discussion of these phenomena, see "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Mechanics," F. R. Moulton, Astrophys. Jour., 1900, p. 109.

† See Moulton's discussion, loc. cit., pp 109–110.

In postulating a mode of growth I have departed radically from the older hypotheses and assigned the gathering of the planetesimals into the nuclei to conjunctions in the course of their orbital movements—not simply or chiefly to the attraction of the nuclei. The nature of the original motions of the planetesimals is therefore a point of vital importance.

I have assumed that the combined outward and rotatory motions to which the formation of the nebula is assigned gave to each individual planetesimal an elliptical orbit about the common center, while their distribution was such as to give a spiral form to the whole. In this I have departed from the common assumption that the arms of the nebulæ marked the courses of the individual constituents.

If the outward and the tangential impulses had been duly balanced, it is believed that circular orbits must have resulted; but neither theory nor observation make it probable that this was often the case. The inevitable inequalities of the two components should give ellipses varying in eccentricity with every variation in their relations. As, however, both the outward and the rotatory components sprang from the same source—the gravitative disturbance induced by the approach to a massive star—there is reason to think that they would be measurably subequal, and that the resulting eccentricities, though large, would not be excessive. This view is in accord with the forms of the spiral nebulæ. These do not present spirally symmetrical configurations of the strictly circuloid type, but broadly elliptical ones, with irregular elements. The development of the present almost circular configuration of the solar system out of such a broadly elliptical, somewhat irregular, spiral configuration involves an evolution in the direction of circularity and symmetry in the course of the aggregation of the scattered matter. How this might have come about I have endeavored to determine.

In the initial stages the orbital ellipses of the nuclei and of the innumerable planetesimals were, by reason of their common origin, rudely concentric. They were, to be sure, more or less discordant in form and in attitude from the effects of unequal projection, of differential expansion of the solar matter when set free by projection, and of the collisions of the constituent planetesimals; but all of this was subordinate to a general concentric arrangement of the elliptical paths. Under the laws of celestial mechanics, these paths must have been constantly modified by the different attractions of the different portions of the nebula. The axes of the orbits must

have shifted, the attitudes of their orbital planes must have varied, and their eccentricities must have been modified. It will suffice to consider the shifting of the major axes of the orbits, technically "the motion of the line of apsides," as that is the most vital factor in the process of aggregation.

So long as the major axes of the orbits were essentially parallel to one another, the bodies would remain apart and aggregation be prevented; but when they became shifted differentially the orbits would be liable to touch, and conjunction be possible if the orbital planes were appropriately related to one another.

The shifting of the lines of apsides is in constant progress in the present system, and must of necessity take place in any such system, as shown by the investigations in celestial mechanics. The shifting is differential and subject to various perturbations, involving alternate movement forward and backward, but the average result is an advance* for all the planets except Venus. At present the line of apsides of quickest revolution is that of Saturn, which completes its circuit in 67,000 years, roundly speaking, while that of Neptune requires 540,000 years, and that of the earth a little more than 100,000 years.† In the course of time the major axis of each orbit is thrown athwart that of its neighbors, and whenever the longer axis of the smaller orbit is equal to the shorter axis of the larger orbit, and the planes of the orbits are properly related, collision is rendered contingent. Actual collision is dependent, of course, upon the bodies reaching the crossing of their paths at the same time. The planes of the planetary orbits now lie near to one another and are presumed always to have done so. These planes, though not necessarily the orbits, intersect one another, and the lines of intersection are shifting, so that in time all assignable intersections are realized. Under these conditions the mechanics of such a system furnish the requisite contingencies for collisions between the planetesimals and the nuclei if sufficient time be granted.

The collisions of isolated planetesimals with one another may be neglected, for it is uncertain whether the planetesimals would rebound from one another or would unite; probably the former when they were highly elastic, and the latter when inelastic; and probably much would also depend on their velocities and their modes of impact; but in any case the result would only affect the size and number of the planetesimals. The important consideration is the

---

* Celestial Mechanics, F. R. Moulton, p. 245.

† Young's General Astronomy, p. 313.

impact of the isolated planetesimals upon the planetary nuclei. In this case the usual result must apparently be the capture of the planetesimals by the nuclei; and with each capture the power of further capture would be augmented.

When two bodies in concentric elliptical orbits unite, their conjoined mass must move in an orbit that is intermediate between the two previous orbits, and this new orbit, in all cases investigated, is less eccentric than one of the previous orbits, and may be less eccentric than both. While a rigorous mathematical demonstration that this is universally true has not been found, it appears to be true for all normal cases falling within the problem in hand; and if so, it follows that the union of an indefinite number of orbits progressively reduces the resulting orbit toward circularity. In application there arises the obvious corollary that planets that have grown most have in general lost most of their primitive eccentricity, and those that have grown least most nearly represent the original eccentricity. This has a significant application to the planets of the solar system, as will appear later.

When the slowness of the motion of the line of apsides and the only partial coincidence of the planes of the orbits at any one time are duly considered, it is evident that the contingencies of collision for the entire number of planetesimals will be spread over a protracted period, and that collisions can succeed one another rapidly only as the immensity of the possible number insures this. Individually, the chances of collisions are remote and infrequent, but as the numbers involved at the outset were prodigious, the impacts upon a given nucleus in a given time may have been numerous. In the nature of the case, the impacts must have declined in frequency after the greater number of planetesimals had been gathered into the nucleus.

The rate of accretion is a matter of radical geological importance; indeed, it is, in some measure, the most critical feature of the whole nebular problem, for the rate of accretion determines whether the average temperature on the surface of the growing body will be high or low. The surface temperature is not determined by the total heat produced by the collisions, but by *the heat produced in a given time*, which, in turn, is determined by *the frequency and force of the collisions on a given area*. If the succession of collisions on a given square mile was not rapid enough to generate heat beyond the concurrent radiation from the square mile, a high average temperature for the whole could not be reached, however great the sum total of

heat generated in the course of time. It is to be noted that the heat generated after a solid nucleus was developed must have been superficial and hence readily radiated away. While the nuclei remained assemblages of small bodies, perhaps gaseous in part in the larger ones, planetesimals from without may have penetrated to the interior and there developed heat not so readily lost. But this state is only assignable to the early stages.

A further consideration bearing upon the critical subject of temperature is the manner of collision. Since all the planetesimals and planetary nuclei were revolving *in the same direction* about the solar mass, the collisions were all overtakes, and could have been violent only to the extent of their differences of orbital velocity, modified by their mutual attractions. These velocities are of a much lower order than the average velocities of meteoritic collisions. Many of the overtakes would obviously be due to differences of velocity barely sufficient to bring about an overtake. When the relative mildness of impact is considered in connection with the intervals between impacts at a given spot, the conviction can scarcely be avoided that *the surface temperature would not necessarily have been high.* It seems probable that it would have been moderate throughout most of the period of aggregation, and certainly so in the declining stages of infall.

The development of the hypothesis has now reached a point where it can be tested. It happens to be a point where all hypotheses of this class have been supposed to be fatally at fault. The crucial feature lies in *the direction of rotation* which would result from the gathering in of matter in this way. At the same time, the bearing of the discussion broadens, for this vital question of direction of rotation attaches to all forms of aggregation of independent bodies moving in orbits about the common center of a system. For example, if the evolution of the solar system be supposed to start with a gaseous spheroid of the Laplacian type, and to proceed in the manner postulated by Laplace until the planetary rings were formed, and if then the velocities of the molecules resulting from mutual impact carried them beyond the gravitative control of the rings, so that they were scattered and revolved independently around the central mass, the hypothesis of their aggregation would be as much subject to the test of rotation as the special hypothesis now under consideration. So, too, if instead of forming definite rings, the molecules were separated from the supposed gaseous spheroid, one by one, as seems more probable than separation by rings, their aggregation would be

equally open to the supposedly fatal weakness. So indeed is the concentration of any kind of an assemblage of discrete matter in which the individual molecules or aggregates revolve independently.

The supposed fatal difficulty is as follows: In a ring revolving *as a unit*, as the Laplacian rings are supposed to have done, the outer part moves faster than the inner part, and so, if a planetary ring breaks at its weakest point and gathers into a globe about the center of its cross-section, it will rotate *forward*. If, on the other hand, the particles of the ring revolve *independently*, the inner ones must move faster than the outer ones, and if they collect about the middle part, it has been held that the rotation must be *retrograde*.*

By way of exception, to meet the singular cases of Uranus and Neptune, it has been suggested that if the matter of the planetary rings, revolving as units, happened to collect about some point other than the center of the cross-section, the foregoing conclusions would not hold; but if the matter were drawn together by gravity simply, as usually supposed under the Laplacian hypothesis, it is not evident why it should not collect about the middle part.

Now, as a matter of fact, the six inner planets and their satellites rotate *forward*. The satellites of Uranus revolve backward in a plane inclined 82.2° to the ecliptic; those of Neptune also revolve backward in a plane inclined 34.5° to the ecliptic. The rotations of the planets themselves have not been determined. These exceptional inclinations and rotations have been interpreted as very oblique or partially overturned rotations. Accepting the foregoing premises, the prevalence of direct rotation has been regarded as strongly confirmatory of an origin from gaseous rings rotating as units, and as strongly adverse to accretion from bodies revolving independently. The force of this line of reasoning has apparently been felt to be so strong as to be essentially fatal to the latter conception. It therefore requires critical consideration.

The reasoning is good *for the special case cited*, that of a symmetrical ring of perfectly circular form, in which the inner bodies in uniting with the outer ones are supposed to strike their inner sides. To bring about this delicate adjustment systematically, the orbits must remain closely concentric and the inner ones must be enlarged, or the outer ones be reduced so that they will approach concentrically to within the sum of the semi-diameters of the bodies to be united. If planetesimals were arranged in strictly circular concentric orbits,

* For ampler statements of this difficulty, see Faye, Sur l'Origine du Monde, pp. 165, 270–281, 1896; also Young's General Astronomy, pp. 518–520.

and were separated from one another at the distances the case requires, the mechanics by which they could be brought into this special mode of collision consecutively is not evident and has not been explicitly pointed out. It is certain that their union into a spheroid would not be by any means the simple, direct, and rapid process usually assumed.* On consideration it will be seen that the postulated case is a very special and quite artificial one, for all the present planetary orbits are *elliptical* and are by no means strictly concentric.

It becomes evident, on studious consideration, that in any case which could probably arise from any actual antecedents, the planetesimals *must have had elliptical orbits;* for even if they arose from a gaseous ring of the Laplacian type the rebounds of the molecules as they collided and separated must have given rise to non-concentric elliptical orbits. Even in this case the measure of the eccentricities must probably have been many million times the sum of the semi-diameters of the particles. In the case of planetesimals derived from a spiral nebula, the orbits are necessarily assigned very notable eccentricities. In all these cases the most available mode of aggregation, if not the sole practicable one, lies *in the crossing of the orbits* brought about by the constant shifting of their major axes, as already set forth.

Now, a planetesimal in a smaller elliptical orbit can come into contact with a planetary nucleus in a larger orbit *only* when a more or less aphelion portion of its orbit coincides with a more or less perihelion portion of the larger orbit of the nucleus, and a planetesimal in a larger orbit can come into contact with a planetary nucleus in a smaller orbit only when a more or less perihelion portion of its orbit coincides with a more or less aphelion portion of the nucleus' orbit.

Now, the vital point lies in the fact that *at the point of collision* the body in the *smaller* orbit is moving *slower* than the one in the larger orbit, though *on the average* it moves the faster.

If the body in the outer orbit were always to strike the outside of the body in the inner orbit, the impact would contribute to forward rotation; but the orbits may cross one another, and the body in the inner orbit may have passed the crossing before it is overtaken by the body in the outer orbit, and so the inertia of the overtaken body may be felt on the *outer* side of the nucleus and tend to produce retrograde rotation. It is, therefore, necessary to take account of two

*This has been discussed mathematically by F. R. Moulton: An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics. Astrophys. Jour., Vol. XI, pp. 115-126, 1900.

opposite classes of effects and to estimate the residual influence of all probable collisions. It will be seen at once that this residual influence must be far less in magnitude than the sum of the forces of all impacts, for the opposing classes neutralize one another, and hence the resulting rotation is likely to be relatively low, though the total force of impact be great. It is further evident that the result might have varied considerably in the different planets, and this is in concordance with the varying rotations actually presented by the several planets.

It is still further obvious, on inspection, that the greatest differences of velocity, and hence the greatest rotatory effects, must occur in the extreme or limiting cases of collision that occur at the perihelion and aphelion points of the nucleus' orbit; for, where the orbits have more nearly the same dimensions and the crossings are at points intermediate between these extremes, the differences of velocity are less and the rotatory effects less, whatever their phases.

By graphical inspection of all probable cases, it may be seen that the possibilities of overtake favorable to forward rotation exceed those favorable to retrograde rotation. This holds true on the assumption of an equable distribution of planetesimals, which may fairly be assumed as an average fact, but not necessarily as always the fact; and hence the conclusion is not rigorous, and a backward rotation is not impossible. From the nature of the case, a varying rotation for the several planets is more probable than a nearly uniform one.

It is also obvious that the impacts on the right and left sides of a growing nucleus, as well as those on the outer and inner sides, might be unequal, and hence *obliquity* of rotation of varying kinds and degrees might arise. As the solar system presents these variations, the method of accretion here postulated seems to lend itself happily to the requirements of the case.

There is a supplementary factor arising from *the order in which the contingency of collision arises.* If a planetesimal is subject to two equal contingencies of collision with the planetary nucleus of opposite effect, it is obvious that the one which it first encounters has a better chance of realization than the other; for if the first is realized the second loses its chance. Now, by inspection it may be seen that, in the shifting of the inner orbits, it will be possible for the planetesimals to collide with the inner side of a nucleus earlier than with the outer side, and hence forward rotation is favored. So, also, by an examination of the orbits of the outer planetesimals a similar fact

is made obvious. Thus the *order* in which the possibilities of collision are brought into effect favors direct rotation.

From the previous discussion it will be seen that a planetary nucleus gathers planetesimals that have orbits both smaller and larger than itself, and hence in effect it sweeps a space both outside and inside its own zone. The breadth of this space is dependent on the eccentricity of its own orbit and on the eccentricities of the orbits of the planetesimals it gathers in on either hand.

It is obvious that there may have been two or more nuclei originally within the same zone. If one of these was notably smaller than the other, it might be picked up by the latter the same as if it were a planetesimal. Two of equal size might perhaps unite, though this would not necessarily take place. Two nuclei in nearly the same zone must feed upon the same belt of planetesimals and must mutually interfere with one another's growth. If there were little difference in their masses at the outset, that one which was best spaced out from the nuclei in neighboring zones would be likely to become dominant by superior growth, for it would have a better feeding-ground, so to speak. Even a nucleus that was smaller at the outset, if well separated from large competitors might become the dominant one by a better growth.

If there were originally many nuclei of minor mass and if these were much scattered, especially if the planes of their orbits were diverse, the dominance of any one might be avoided and a scanty growth of all result, as in the case of the asteroids.

It seems to be a sure inference that in the process of growth the nucleus must have *worked toward the center of the zone* from which it gathered, as a consequence of the superior feeding on the richer side. For example, if more planetesimals were picked up on orbits smaller than its own, its orbit must have grown smaller as a mechanical result of the accretion, for a new orbit, arising from the union of two bodies, is intermediate between the two previous orbits, and hence smaller than the larger one. If more planetesimals were picked up on the outer side, the orbit of the nucleus must have grown larger. The nucleus, therefore, must have worked toward the center of the richer feeding-ground, or in average cases of equable original distribution, toward the ground not preyed upon by other nuclei.

The foregoing processes tended toward a selection of nuclei for dominance and to an automatic spacing out of the successful nuclei. This process, if our hypothesis be true, should find verification in the

actual distribution of the planets and be an explanation of it. This distribution should correspond to the eccentricities of the nuclei, modified by the proportions of planetesimals of larger and smaller orbits gathered in by them. Assuming these to have been somewhat equable, the planetary distribution should be roughly proportional to the eccentricities of the nuclear orbits. As a basis for inspection, let it be supposed that the collecting zone of each planet reaches halfway to its neighbor on either hand, and let the eccentricity of the orbit of each nucleus be such that the nucleus itself shall sweep its whole collecting zone, which is more than the case absolutely requires. The following are the eccentricities so derived compared with present eccentricities :

| | Assigned eccentricity. | Present eccentricity. |
|---|---|---|
| Nucleus of Mercury | 0.25± | 0.2 |
| Venus | .21 | .006 |
| Earth | .2 | .017 |
| Mars | .28 | .093 |
| Asteroids (mean) | .33 | .38 downward.* |
| Jupiter | .336 | .048 |
| Saturn | .366 | .056 |
| Uranus | .37 | .046 |
| Neptune | .38± | .009 |

There being no known planet outside of Neptune, the method can only be applied to it by an arbitrary assumption regarding its outside collecting area. It may be reasonably assumed that the nucleus of Neptune represented the head of the protuberance, so to speak, and that its accretion was essentially all on the inner side, which would draw its orbit inward, according to the principle above stated. This may account for its anomalous spacing out. There being no known planet inside Mercury, the eccentricity assigned it is also in a measure arbitrary.

With these qualifications, it will be seen that the assigned eccentricities are quite harmonious, and on the whole they indicate a progressively greater original eccentricity from within outward. By comparison with the existing eccentricities it will be seen that the assigned original ones are much the more consistent. The reason for this, under our hypothesis, is close at hand. According to the principle of evolution from eccentricity toward circularity, stated above, the greater the accretion the greater the progress toward circularity. This is qualified somewhat by the perturbations which

* Mean about 0.15.

the planets create in one another's orbits and by the special conditions of aggregation, but remains essentially true. For the large planets that have dominated their collecting zones and presumably swept them thoroughly, the reductions of eccentricity are subequal. For the very small bodies that presumably grew but little, *the eccentricities remain large*, for the greater part. For example, the eccentricity of Mercury, the smallest of the planets, remains more than twice that of any other planet. Mars, the next smallest in size, comes next in eccentricity among the planets, while the asteroids, which probably grew but little, have high eccentricities, as a rule. Their orbits have doubtless been not a little disturbed by the great influence of their powerful neighbor, Jupiter, and a rigorous application of so general a law as the one under consideration can not be made to the details of their orbits, but the tenor of the facts is very suggestive. The highest eccentricity, 0.38, is as high as the highest eccentricity assigned to the original nuclei of the planets. Of the seventy asteroids whose diameters are fairly well known, the half that are larger and presumably have grown most have less eccentric orbits by 13.7 per cent than the half that are smaller and presumably have grown less. Of the orbital elements of 278 asteroids examined, the half having the lowest inclination to the common plane of the system, and so best suited for accretion, have eccentricities 21.9 per cent less than those of greater inclination. The orbits of Neptune and Venus are exceptionally circular, the former, perhaps, on account of its outermost position and mode of accretion, as previously suggested; the latter for reasons not obvious. Rigorously consistent results can not be expected from such antecedents as are postulated in a case of this kind, and the mutual perturbations of the planets introduce variations from the average eccentricities. The degree of consistency noted is, perhaps, to be regarded as much more remarkable than the departures from it. If this view of the spacing out of the planets be entertained, a rational law may be substituted for the purely numerical formulation known as Bode's law, viz, that the spacing has been derived from a fairly consistent variation in the primitive eccentricities of the planetesimals and nuclei of the parent nebula, in which the outer were symmetrically greater than the inner.

It has thus been my endeavor to develop the hypothesis into sufficient detail (1) to furnish a large number of points of contact with known phenomena and with recognized mechanical principles to facilitate testing its verity by those relations, if not now, at least in

the early progress of investigation; (2) to furnish a basis for deducing the hypothetical stages of the earth that preceded its known history, and for drawing thence inferences as to the conditions of the interior which the earth inherited from the mode of its birth; and (3) to stimulate inquiry into the elements involved. In short, I have endeavored to give the hypothesis a working form under the conviction that so long as the complicated elements involved remain so imperfectly determined as at present its working value is its chief value.

To bring out the geological bearings of the planetesimal hypothesis, I have given considerable time to a study of the probable stages of growth of the early earth, of the time and mode of introduction of the atmosphere and hydrosphere, and of the initiation of the great topographic features, together with the leading modern processes. While it is clear that there may be a somewhat wide range of sub-hypotheses relative to these stages as to the earlier, it was thought best, as before, to develop a single line definitely. The line selected is in direct sequence to that chosen for the earlier stages, so that there should be no resting back on factors not previously introduced, and so that the whole should be consistent. Of course, the complete scheme contemplates the development of the alternative sub-hypotheses.

Following the postulates of the previous sketch, a nebular knot is assumed to have been the nucleus of the growing earth. It has not been thought important to consider at much length the special state of organization of the material of this nucleus, since by assumption it constituted but a minor part of the grown planet, and its ultimate condition would probably be that of the dominant mass, or, if not, would be so deeply central as to have little geologic importance. Assuming that the nuclear mass was quite small, it is inferred that it was composed chiefly of matter of high molecular weight, since light molecules would be liable to escape because of their velocities. The nucleus is supposed to have been originally an assemblage of planetesimals grouped together by their mutual gravity, and to have passed gradually into a solid mass in connection with the capture of outside planetesimals. As the planetesimals were solid aggregates in the main, and only partially elastic, their collisions are assumed to have destroyed their orbital motions in a certain proportion of cases and to have led to their collection at the center. In other cases the orbital motions were doubtless increased, but any planetesimals which were thus temporarily driven away were subject to subsequent capture.

As the solid nucleus thus formed may not have been massive

enough to control a gaseous envelope in its earlier stages, a possible atmosphereless stage is to be recognized. Just how massive a planetary body must be to hold permanently an appreciable atmosphere is not accurately computable at present, because of the uncertain value of some of the factors involved.* A fairly safe conclusion may perhaps be drawn from known celestial bodies. The moon ($\frac{1}{81}$ of earth's mass) has no detectable atmosphere, nor has any smaller body, whether satellite or asteroid, so far as known. Mars ($\frac{1}{9.34}$ of earth's mass) has an appreciable, but apparently quite limited, atmosphere. The limit between atmosphereless and atmosphere-bearing bodies probably lies between the two—*i. e.*, roundly between one-eightieth and one-tenth of the earth's mass. The mass of Mercury, unfortunately, is not known with satisfactory accuracy, because it has no satellite and offers no other ready means of determination. Values all the way from one twenty-sixth to one-ninth of the earth's mass have been assigned. Mercury gives no distinct signs of atmospheric refraction, and its reflection of light (albedo) is very low, even lower than that of the moon, and, like that of the moon, is relatively much stronger for surfaces normal to the line of incidence and of vision than for those oblique to it, which is characteristic of a rough surface. All this implies the absence of an atmosphere and hydrosphere of sufficient value to give effective reflection of themselves or to develop a good reflecting body by smoothing down the surface and filling up the pores. On the other hand, certain lines of the planet's spectrum have been thought to imply the presence of water-vapor; but this is not conclusive. The probabilities seem to be that Mercury has no atmosphere that is effective as a weathering or degradational agent, which is the point of geologic interest. This brings the limit of appreciable atmosphere much nearer Mars

* The following papers bear upon this subject: G. Johnstone Stoney: On the Cause of the Absence of Hydrogen from the Earth's Atmosphere, and of Air and Water from the Moon; Roy. Dublin Soc., 1892. G. Johnstone Stoney: On Atmospheres upon Plants and Satellites; Trans. Roy. Dublin Soc., 2d series, 6, 1897; *ibid.*, 1898, p. 305. T. C. Chamberlin: A Group of Hypotheses Bearing on Climatic Changes; Jour. Geol., vol. V, 1897, p. 653. G. Johnstone Stoney: On the Presence of Helium in the Earth's Atmosphere and its Relation to the Kinetic Theory of Gas; Astrophys. Jour., vol. VIII, Dec., 1898, p. 316. S. R Cook: On the Escape of Gases from Planetary Atmospheres According to the Kinetic Theory; Astrophys. Jour., vol. XI, Jan., 1900, p. 36. G. Johnstone Stoney: On the Escape of Gases from Planetary Atmospheres According to the Kinetic Theory, No. I; Astrophys. Jour., vol. XI, May, 1900, p. 251; No. II, *ibid.*, June, 1900, p. 325. G. Johnstone Stoney: Note on Inquiries as to the Escape of Gases from Atmospheres; *ibid.*, vol. XII, Oct., 1900, p. 201.

than the moon and justifies the provisional conclusion that if the young earth had no more than one-twentieth of its present mass it probably possessed no atmosphere of appreciable geological efficiency, but that when it had gained one-tenth of its present mass (radius probably about 2,100 miles) an appreciable, though relatively slight, atmosphere surrounded it.

When the growing earth reached a mass sufficient to control the flying molecules of atmospheric material, there were two sources from which these could be supplied for the accumulation of an atmosphere, an external and an internal one.

By hypothesis, all the atmospheric and hydrospheric material of the parent nebula which was not gathered into the aggregated planetesimals remained as free-molecular planetesimals. While the planetary nucleus was small it probably could not gather and hold the lighter molecules, even when they collided with it, except as this was done by occlusion or surface tension, in which case they did not form an atmosphere; but when the growing earth reached the requisite mass these free atmospheric molecules were gathered about it and retained as an atmospheric envelope. This would be a more abundant source of supply during the nebular stages than afterward, but by hypothesis it continues to be a source of some supply even to the present time, for the very doctrine that postulates the loss of such high-speed molecules implies their presence in space, subject to capture by bodies capable of capturing them.

In the later stages of organization, and thence down to the present time, the molecules discharged from all the bodies of the solar system were possible sources of atmospheric accretion. Of these the most important were probably volcanic and similar discharges from the small bodies that could not hold gases permanently and discharges from the sun by virtue of the enormous explosive and radiant energies that are there resident.

As the planetesimals were gathered into the growing earth-nucleus they carried their occluded gases in with them, except as the superficial portion might be set free by the heat of impact. There was thus built into the growing earth atmospheric material. So, also, while the nucleus was growing it was subjected to the bombardment of free molecular planetesimals of the atmospheric substances. In its early stages it might not be able to hold these as a free gaseous envelope, but to a certain extent it could hold, by virtue of capillary and subcapillary attraction, such molecules as were driven into the

pores and other interstices of the fragmental surface arising from the infall of the solid planetesimals.

The extent to which gases may be held condensed in small solid bodies is shown by meteorites and igneous rocks to be large. Meteorites carry on the average several times their volume of condensed gas ; so do many, probably most, igneous rocks of the earth. The testimony of the meteorites is peculiarly significant here, for they have traversed unknown depths of space in a practical vacuum, in addition to the vicissitudes of their origin and the heating of their fall. Atmospheric material is carried into the earth's body by them today in quantities that are large relative to their masses. Their testimony becomes the more significant if we accept the view of their origin which makes them but the fragments of small atmosphereless bodies, built up precisely as the early earth was under this hypothesis. This view makes them specific samples of the products of the assigned process.

The atmospheric material thus condensed within the growing earth could become a part of the atmospheric envelope only by extrusion. The assigned modes of extrusion will be considered presently ; meanwhile it may be assumed that these internal gases were given forth progressively and fed the atmosphere.

The contribution made by the external sources of atmospheric material might include any constituent of the ancestral sun that could remain free in the nebula and be picked up and held by the earth. Some portion of the constituents of the present atmosphere may therefore be assigned to this source. In what ratio these constituents were contributed to the nebula probably depended on their proportions in the ancestral sun, or rather their proportions in that part of the ancestral sun that was dispersed to form the parent nebula. Concerning this little can safely be said. Hydrogen is apparently very abundant in the other part of the sun, but it is doubtful whether the earth can even now hold hydrogen in a free state permanently in any large amount. Of the proportions of the common atmospheric constituents in the sun in a free state little is known.

The gases chiefly occluded in meteorites and the crystalline rocks are hydrogen, carbon dioxide, and carbon monoxide in leading amounts, and marsh-gas and nitrogen in small quantities. It is assumed that the gases of the aggregated planetesimals, and hence those of the interior of the early earth, were of the same order of abundance. There is experimental ground for believing that, at the right temperatures and pressures, hydrogen would take oxygen from ferric

oxide (which, from the analogy of igneous rocks and meteorites, may be presumed to have abounded in the earth material) and therewith form water. The gases extruded from the interior should therefore have been largely water-vapor and the carbon oxides, with minor quantities of hydrocarbons and nitrogen. To these might be added such chlorine, sulphur, and other temporary gases as the volatile ingredients of the rock material might contribute through volcanic action; but these chemically vigorous constituents would doubtless soon disappear by union with the rock material. It is probable that carbon monoxide would pass into carbon dioxide, as it does not now accumulate in the atmosphere, although abundantly produced. The marsh gas also disappears in some way.

The material of internal derivation available for the atmosphere, therefore, embraced chiefly water-vapor, carbon dioxide, and nitrogen. Oxygen is now given forth in some abundance by volcanoes, but it is not known whether it really comes from the interior or has merely been carried down from the surface. The reduction of ferric oxide under certain conditions (the reverse of the process by which water is assumed to have been found) might possibly give free oxygen.

The material of external derivation might probably embrace all the atmospheric constituents, but in proportions unknown.

In determining the actual proportions of the constituents of the early atmosphere, the abundance of the supply was probably less decisive than the power of the earth to hold the individual gases. As gravity gradually increased by the growth of the earth from an incompetent minimum, its power to control the heaviest molecules with the lowest velocities was acquired before its ability to hold the lighter ones of higher velocities. According to the kinetic theory, molecular velocities vary inversely as the square root of the molecular weights. Assuming this to be correct, the leading constituents would be held in the following order, it being noticed that *molecules*, not atoms, must be dealt with:

| Molecules. | Molecular weights (in round numbers). | Average molecular velocities at 0° C in cm. per sec. |
|---|---|---|
| $CO_2$ | 44 | 33,259 |
| $O_2$ | 32 | 39,155 |
| $N_2$ | 28 | 41,735 |
| $H_2O$ | 18 | 56,522 |
| $H_2$ | 2 | 169,611 |

The commingling of the gases introduced some modifications of the limitations of retention, and these were favorable to the lighter gases; but the refinements of the case are of no moment here.

Carbon dioxide would be held some appreciable time before oxygen, and still longer before nitrogen, and all these a notable time before the vapor of water. The inference is that the initial atmosphere was very rich in carbon dioxide, for an abundant supply was correlated with a superior power of retention.

The amount of oxygen in the early atmosphere is more uncertain from doubt as to a competent source of supply. Crystalline rocks and meteorites are not known to contain it in a *free* state. As above remarked, it occurs among volcanic gases, but it is not known that it comes from the deep interior. It is detected in the sun and not improbably existed in the nebula, from which it might have been gathered shortly after the accretion of carbon dioxide began. The safer inference seems to be that it was not very abundant relatively in the early atmosphere. This inference may be entertained the more freely because it seems to give the better working hypothesis, for the present large proportion of oxygen may be assigned to the reduction of carbon dioxide by plant action, and the present proportions and those of geologic history seem to come out best on this basis. For the primitive atmosphere there is theoretical need for only enough oxygen to support the primitive plant life until it could supply itself, after which it would produce a surplus.

The amount of nitrogen occluded in rocks and meteorites is relatively small, and it was perhaps a small constituent of the early atmosphere. Owing to its chemical inertness, it may be supposed to have been increasing ever since, and thus to have attained its present dominance. A similar history may be assigned to the other and even more inert elements, argon, neon, zenon, krypton, and helium, of which the supplies seem to have been always very limited.

After the earth acquired the power of holding water-vapor, the supply being abundant, accession doubtless went on for a time as fast as the capacity to hold increased.

The problem of vulcanism assumes a quite new aspect under the planetesimal hypothesis, if very slow accretion without very high temperature be assumed. It has been taken for granted in the preceding statement that there was volcanic action. It is necessary, therefore, to consider how volcanic action may have arisen, and this involves the more radical question how the high internal temperatures of the earth may have arisen if the earth did not inherit its heat from a molten condition arising from a gaseous origin.

The total amount of heat produced by the infall of the planetesimals would undoubtedly be more than sufficient to melt the whole

mass if the heat were all generated at the same instant; but if it were generated in successive moieties spread over a long period and generated at the surface, where readily radiated away, no large amount might be retained, and high internal heat, such as required for vulcanism, might not be assignable to this source. In the present state of knowledge the hypothesis may not unreasonably be given such a form as to make this source partially available by assuming that in the early stages of accretion, while the nebular planetesimals were still relatively numerous, the collisions between them and the nucleus were so frequent as to make the latter hot. It is possible that mathematical inquiries contemplated, but not yet carried out, will show that this was probable, and that a rate of accretion so slow as to give a cool exterior would only come later, after the planetesimals of the feeding zone had been thinned out; but until that can be shown the hypothesis must face the alternative possibility that the collisions did not succeed one another so rapidly as to greatly heat the growing earth body by impact.

An unknown amount of heat may have been inherited from the nebular knot that constituted the original earth-nucleus. This knot is supposed to have consisted of an assemblage of small aggregates made from the heavy molecules of the nebular material; in other words, chiefly the metallic and the rock substances. This is held to be so because these substances would condense to the liquid and solid state at high temperatures, and further because, having low molecular velocities and relatively high gravity, they could assemble and remain associated by mutual attraction, while molecules of low weights and high velocities could not. These assemblages were probably rotatory or revolutionary, but perhaps of a very irregular kind, somewhere midway between a well-organized planetesimal system and a heterogenous gaseous or collision-rebound system, and combining some of the qualities of each. The ingathering of planetesimals from without probably tended to increase the irregularity, and to cause the assemblage to become more and more gas-like in dynamic nature. The matter being rock substance or metallic, and hence partially inelastic, and the collisional velocities generally low, the mode of condensation was probably only in part analogous to that of a gas, but it is possible that an internal temperature not unlike that of a condensing gas might be developed. The young earth may, therefore, have inherited a hot nucleus.

The chief source of internal heat is, however, assigned to the progressive condensation of the growing body as material was added

to its surface. The amount of this condensational heat for the full-grown earth, computed on the best data now available, seems to be ample to meet all the requirements of the known geologic ages, as brought out in the investigations of Dr. Lunn.* That heat arising from condensation *solely* would reach the melting temperature of rock in a body one-twentieth of the earth's mass seems more or less doubtful, but in a body one-tenth of the earth's mass the required conditions would probably be reached. The requisite data are too imperfect for a definite decision of this point at present. If the pits of the moon ($\frac{1}{81}$ of the earth's mass) represent volcanic explosions, and not the infall of planetoids as Gilbert suggests,† it is necessary to postulate in its case conditions very favorable to the generation of heat by compression, or else to assign some notable portion of the requisite heat to the quasi-gaseous condensation of the nucleus, to the collisions of planetesimals, and to the source next to be considered, all of which would necessarily contribute something to the sum total of internal heat.

Another source of heat lay in the atomic and molecular rearrangement of the material after it became entrapped in the growing mass. This was not simply chemical recombination, as usually understood, but molecular readjustment under pressure as well. The planetesimals were aggregated, by hypothesis, in a vacuum of the highest order, and with very slight mutual gravity, and the mode of molecular arrangement was that suited to this extremely low pressure. Under the rising pressure of the earth's interior, new arrangements of the molecules into denser forms with lower specific heats are theoretically assignable, if not inevitable, with the freeing of heat as a consequence. In a sense this is a mode of condensation falling under the previous head, but it is not identical with mere mechanical compression and is not wholly covered by computations based on that.

With the detailed conceptions now developed, the method of volcanic action deduced from the accretion hypothesis may be readily apprehended and the vital part assigned to it in earth history may be realized. The chief portion of internal heat being assigned to compression, the temperature must have been highest at the center, because the compression was greatest there, and must have declined toward the surface.

Pressure itself is probably incompetent to melt rock substances that shrink in solidifying, but the high temperatures generated by pressure

* See statement appended to this report.

† Bull. Phil. Soc. Washington, Vol. XII, 1892, pp. 241–292.

in the deep interior were constantly moving outward into horizons of lower pressures, where the melting-points were lower. As the computed temperature at the center of the adult earth is about 20,000° C.,* there would seem to be no lack of heat, in the later stages at least. The essence of the problem lies in its redistribution and in its selective action.

The material of the interior was originally, by hypothesis, an intimate mixture of planetesimals of various kinds, with such gaseous material as they carried in or entrapped in the process of growth. This material, therefore, presumably ranged from the most fusible to the most infusible of rock material that could take the form of aggregated planetesimals. As some of it was probably the kind that shrinks much in solidifying, and some of the kind that shrinks little, and some possibly of the kind that does not shrink at all in solidifying, it is probable that some of it was brought near or even to the melting-point by pressure, while other parts, intimately intermixed with these, were far from their melting-points. At any rate, the outward flow of heat in such a mixture must bring some parts to fusibility much before the melting-points of other parts were reached. Local spots of fusion must thus arise. To this fusion the entrapped and occluded gases may be presumed to have contributed and to have joined themselves to the fused masses, and to have aided in giving them fluidity.

As the rise of temperature continued, more and more of the mixed material reached the fusing-point, while other material so nearly approached it as to become plastic and permit readjustive movements. In this way fused points are supposed to have been permitted to join one another and to move in the direction of least resistance. The static pressure from the earth body itself was always greatest below and least above, but was nearly constant for any given short period. The stresses arising from the differential tide-producing attractions of the sun and moon were also greatest below and least above, but were *periodic*, stress and relief following one another in semi-daily succession, giving a kind of kneading process. These interior stress differences are thought to have pressed outward the fused vesicles, causing them to unite and form threads or stringlets, insinuating themselves through the more refractory portions that remained solid, and at length developing into tongues of some volume. As these liquid threads or tongues rose to higher horizons of lower pressures,

* See the investigations of Dr. Lunn.

and hence of lower melting-points, they carried with them a certain surplus of heat above that required to maintain their liquidity in the new horizon, and this surplus was available for melting or fluxing their way. They were at the same time, however, subject to loss of heat by contact with surrounding rock of lower temperature. They were thus probably at the same time taking up fusible material met in their path and depositing old material as it became less adapted to remain fluid under the new conditions, either because it had reached the point of its saturation in the mixed rock solution that had been developed or had cooled to its point of congelation. The liquid thread was thus presumably taking on and giving up material continually as it worked its way outward, the process always being selective and involving the retention of the more soluble or more fusible portions and the rejection of the less soluble or more refractory portions. Since the included gases may be safely reckoned with the former class, there was a selective accumulation of these, and the ascending liquid became densely charged with them. To this ascensive process those substances whose weight overbalanced the differential pressure, such as metallic iron and possibly the heaviest silicates, may be regarded as forming exceptions.

Theory does not require that these threads should all succeed in reaching the surface; indeed, it does not require that any should in the initial stages, before compression had developed a great excess of heat in the central parts. The molten threads should simply rise until their excess of heat, their working capital, was exhausted, when they would return to the solid state and constitute tongue-like intrusions. In doing this they would contribute heat to the tracts which they invaded. This, in addition to conduction, was a mode of conveying the intenser heat of the compressed central regions to the higher horizons, where the original temperature was lower and the fusing-points lower. The failure of the earlier threads to reach the surface would thus be a means preparatory to the greater success of later ones. The conditions for penetration would probably be favorable up to the horizon where the temperature ceased to be higher than the surface melting-point. Below this the retention of the solid state was wholly due to pressure, the temperature being above the surface melting-point. When the threads reached the higher zone, in which the temperature was appreciably below the surface fusing-point, the conditions were clearly adverse, and further ascent was dependent on a sufficient excess of heat brought from below to maintain the liquid state while this adverse tract was

being traversed. It was probably also dependent on a fluxing power adequate to enable it to fuse its way through the solid zone of continuous rock that lies below the fracture zone. When it reached the latter, hydrostatic pressure and the inherent expansive force of its gaseous content would probably control its further course in the main.

Now having in mind that, at the early stage under consideration, the earth was growing, that its internal self-compression was increasing apace with its growth, that the heat was rising with the compression, that the temperature was highest at the center and graded toward the surface, and that it was also carried outward by the liquid threads, the succeeding steps may be followed easily.

The outer part of the young earth was made up of the recently fallen planetesimals and their fragments, and no doubt had a much-broken, open texture. If there was as yet no atmosphere nor hydrosphere, as in the case of the moon, there was no effective process for the wash of fine fragments into the interstices of the coarse, or, what is more important, for the solution of the material at the surface and the cementation of that below into a solid mass, as is the present habit on the earth ; in other words, there was no effective healing process to unite the broken fragments. The porous clastic zone must therefore have extended downward to a depth at which gravity was able to force the fragments into continuity by its crushing effects. In a small body this zone would be deep.

When the rising lava tongues reached this outer fragmental zone, fluxing was no longer required, as they could force their way by insinuation and by mechanical displacement. It appears almost certain that in the upper part of such a fragmental zone the interstices would make up a sufficient part of the volume of the aggregate mass to reduce its average specific gravity to a figure below that of the penetrating lava, even though the latter might be made up of lighter material inherently, and was also hot and liquid. The earliest tongues of molten material are supposed, therefore, to have generally lodged within the fragmental zone, taking various plutonic forms, as dikes, sills, laccoliths, and batholiths, and to have there given off their gases, which, more or less concentrated and condensed, doubtless not infrequently forced an exit to the surface by blowing away the overlying fragmental material. The slight coherence of this material, the low gravity of the young earth, and the absence or scantiness of a resisting atmosphere should combine to give to the pit-forming effects extraordinary magnitude, such, perhaps, as the moon exhibits.

It is not necessary to the hypothesis to suppose that volcanic action was an essential preliminary to the acquisition of an atmosphere, nor that it came into function before the earth acquired an atmosphere, for the initial atmosphere may have been supplied from external sources. The apparent vigor and the wide prevalence of volcanic action on the moon, if its pitted surface means vulcanism, as well as the glassy material found in meteorites, whose origin is referred preferably to small atmosphereless bodies, favors the view that the internal gases were given forth abundantly before the earth grew to a mass sufficient to hold them. If this were true, an ample source of atmospheric supply was ready and waiting when the earth first acquired sufficient gravity to clothe itself with a gaseous envelope.

When the increasing water-vapor of the growing atmosphere reached the point of saturation, it is of course assumed to have taken the liquid form and became a contribution to the hydrosphere. Probably condensation had occurred within the fragmental zone long before the external atmosphere reached saturation. The hydrosphere, therefore, probably had its birth under ground, and so long as the fragmental zone retained its highly porous condition it was what its name implies, a veritable sphere or spheroidal layer. As accumulation went on, it is assumed to have risen to the surface, and doubtless first appeared in the innumerable pits resulting from the previous volcanic action and in the depressions resulting from other deforming agencies. Its surface deployment is, therefore, pictured as a growth from innumerable lakelets scattered with unknown promiscuousness over the face of the young planet, into more and more enlarged and confluent bodies, until at length they developed into the vast irregular oceans of to-day. This evolution is of fundamental geologic importance, for it involves the origin of the ocean basins and of the continental platforms, and these constitute at once the grand topographic features of the globe, the great integers of deformation, and the controlling physical factors in the evolution of life. The evolution of the ocean basins and the continental platforms under this hypothesis is, however, exceedingly simple.

With the acquisition of an atmosphere and a hydrosphere, the conditions for weathering were present, and all those attendant processes of a gradational nature which constitute the dominant surface work of to-day.

For the present study, two features of these gradational processes overshadow all others, (1) *the leaching action of the atmospheric waters*, and (2) *the relative protection of the water bodies*. The essence of the

leaching process is this: Through the action of the atmosphere and atmospheric waters the basic material is more largely dissolved and carried away than the acidic. When the weathering is thorough, the residue is chiefly quartzose sand—if the original rock contained quartz—and various residual earths and clays which are essentially silicious silts and aluminum silicates, with a low percentage of the basic oxides. If these earths and clays are turned back into crystalline rocks by metamorphism, they form acidic schists or gneisses, while the quartzose sand becomes quartzite. The material borne away in solution consists mainly of compounds of the alkalies and alkaline earths. A part of this is redeposited within the zone of the hydrosphere beneath the land, and a part is borne to the sea and remains in solution or is deposited beneath it. Although some decomposition takes place in the zone of the hydrosphere beneath the land, and some also beneath the permanent water bodies, it is clearly less than that which takes place in the zone of the atmosphere, and this difference in the sum total of work done is all that need here be considered. There can be no question that the land areas lose by leaching and the water areas gain correspondingly. The general effect is an increase in the acidity and a reduction in the specific gravity of the land material. This includes the land wash deposited on the borders of the continents.

Now, when the growing hydrosphere crept up to the surface and covered the lower tracts a selective action of this kind began. The surface material of the areas that remained exposed lost more of its basic than of its acidic constituents, while the submerged material lost less and perhaps gained something by the redisposition of the matter borne in from the land. As the planetesimals were being gathered in on land and water alike, those that fell on the land suffered some atmospheric action, while those that fell into the water were mainly protected from it. As this differential action affected each successive layer of growth after the accumulation of surface waters began, the specific gravity of the land areas came to be less than that of the submerged areas.

It is not the temporary specific gravity that resulted from the change of physical state involved in disintegration that is to be considered here, but rather what may be termed the inherent or permanent specific gravity—*i. e.*, the specific gravity that would be retained after any metamorphism which the material might probably suffer in the future had taken place. So, likewise, it is not the temporary chemical combinations arising immediately from the weathering, but

the potential future combinations that are significant. For example, any rock likely to arise from the residual sands, earths, and clays by any probable metamorphism, or even by remelting, would have a lower specific gravity than the original average rock, or than any rock likely to be developed from the alkalies and alkaline earths removed by the leaching process in connection with the original rock. The leaching of the land material had, therefore, a permanent effect on its specific gravity—an effect not eliminated by any probable change resulting from its burial under late accumulations. The segments built up by accretion on the land were hence lighter than the segments built up under the waters, and the difference increased as the segments grew in thickness.

It follows from the greater weight of the water-covered segments that the compression beneath them, as they became more and more weighted with incoming material, was greater than the compression beneath the land segments, and hence the water-covered areas were depressed relatively more than the land areas. The waters drawn in upon the depressed segments augmented the depressing effects due to difference in specific gravity.

It is not necessary to suppose that there was at the outset a general or continuous covering of certain large areas by water and a general and continuous prevalence of land in other regions, but merely that over certain portions of the globe water areas were more abundant than over other areas. Where water predominated it may at first have taken the form of numerous small bodies. Such areas of prevalent water would, on the average, become heavier than other areas, and hence, acting more or less as units, would become more depressed. This excess of depression would extend the water-covered areas, draw water away from the areas less depressed, and this water would add its weight to the previous excess, and so by progressive and cumulative action develop the great water areas and differentiate them from the chief land areas. The tendency would always be toward the more complete unification of the land and water areas respectively.

So long as the earth continued to grow appreciably by accession, the water areas should continue to grow larger and deeper and the land areas narrower and higher, so far as this one process is concerned. The wash from the land tended to build its borders out into the water basins and other influences modified the results, but the deepening and spreading of the water basins is believed to have been a markedly dominant process during the earth's growth. After

growth ceased and modern processes became dominant a more nearly balanced relation of sea and land is thought to have ensued, with a closer approximation to constancy.

The amount of the original depression of the areas occupied by the water is assumed to have been slight, and, we prefer to think, accidental, so to speak. There may have been systematic causes that determined the relative depression of certain broad tracts and the relative elevation of others, such as some systematic difference in the infall, or some rotational change, or some inherent tendency to shrinking in certain particular ways, as, for example, that held by advocates of a tetrahedral earth, but it is not clear that the actual distribution of depressions and elevations points to such systematic agencies. The elevated and depressed tracts of the moon seem to have a distribution quite unlike those of the earth; and those of Mars, if the lighter and darker areas are correctly interpreted as elevated and depressed tracts, are quite different from those of either earth or moon. Each seems to be a law unto itself, if such irregular distributions can be styled laws at all. My hypothesis requires nothing more than the inevitable slight differences of growth, of volcanic activity, of compression, and their joint effects. Starting with only such slight differences as were sufficient to give preponderance in large tracts in favor of the water or of the land, the selective and self-propagating nature of the process may have done the rest.

If it be assumed that the earth's growing hydrosphere appeared at the surface when our planet had attained the mass of Mars, whose radius is about 2,100 miles, the subsequent growth would form a shell about 1,900 miles thick. It is not altogether certain that Mars bears water bodies on its surface; but the areas of greenish shade environed by a surface generally ruddy, the polar white caps ("snow caps") that come and go with the seasons, and the apparent occasional presence of clouds, not to appeal to the evidence of aqueous absorption lines in the spectrum reported by some good observers, but unconfirmed by others, lend some support to the opinion that water is present, though perhaps not in the form of definite water bodies.

It has been inferred from the almost complete, and sometimes total, disappearance of the polar white caps in summer, and from other phenomena, that the climate of Mars is phenomenally mild, considering its distance from the sun. This has been regarded as all the more puzzling because of the scantiness of the Martian atmosphere, but is what might be expected if Mars' atmosphere is like that

assigned the earth at a similar size, *i. e.*, composed largely of the heat-absorbing carbon dioxide.

Without attempting to fix the precise stage, it is not unreasonable to assume that surface waters had begun their accumulation upon the earth's exterior while yet it lay 1,500 to 1,800 miles below the present surface. The present difference between the radii of the oceanic basins and the radii of the continental platforms is scarcely 3 miles, on the average; so that if the continental segments be assumed to be in approximate hydrostatic equilibrium with the oceanic segments today, as seems highly probable, the selective weathering process brought about a difference in depression of only 1 mile in 500 or 600 miles, or about one-fifth of 1 per cent. We appear, therefore, to be laying no heavier burden upon weathering than it is competent to bear. It might well be thought to do much more, but the process of weathering is slow, and as new material was constantly falling in and burying the old, partial alteration was all that could take place; and, besides, a part of the basic material leached from the surface was redeposited beneath the ground water of the land and in landlocked basins and was not lost to the continental segments.

Not only is the evolution of the great abysmal basins and of the continental platforms thus assigned to a very simple and inevitable process, but there is therein laid the foundation for subsequent deformation of the abysmal and continental type.

There is no direct evidence as to the time or the method of the introduction of life upon the earth. The earliest legible record of life in the form of fossils bears evidences of great advances in evolution along many divergent lines. The inference is therefore imperative that the initial forms of life had been introduced long before, or else that an evolution quite out of harmony with that which succeeded took place in the unknown interval antecedent to the record. Whence the life was introduced is also quite unknown. The speculation that it might have been brought to the earth from some other celestial body by a fragment in the form of a meteorite is merely a refuge from supposed geological, biological, and philosophical difficulties—a merely temporary refuge in the face of prodigious improbabilities, for it only throws back the problem of life genesis without solving it. There is nothing in known meteorites save, perhaps, the existence of hydrocarbons equally assignable to inorganic sources, to indicate that they came from worlds with atmospheres and hydrospheres suited to maintain such life as the

problem presents. On the contrary, there is the best of grounds for believing that meteorites came from bodies in which the essential conditions of life were wanting; for, besides the absence of free oxygen and water, there is an absence of the products assignable to weathering and to those rock changes that spring from the presence of an atmosphere and hydrosphere. These embrace a large portion of all known rocks in the outer part of the earth, such as are characterized by quartz, orthoclase, the acid plagioclases, the micas, the amphiboles, as well as the sedimentary rocks. The absence of these in the meteorites is peculiarly significant because of their abundance in the earth. The hypothesis of the foreign importation of life encounters a special difficulty under the planetesimal hypothesis, in that the planets were all forming at the same time. Under the other hypotheses the outer planets may have formed earlier than the inner ones, and an earlier evolution of life may have taken place in one of the older planets, whence a transference to the earth is barely conceivable. Under the accretion hypothesis even this is scarcely a tenable refuge, and transfer from some other stellar system is the only obvious recourse.

The planetesimal hypothesis affords an undetermined lapse of time between the stage when conditions congenial to life were first possible and the stage when the first fairly legible record was made in the Cambrian period. To this unmeasured period the whole pre-record evolution of life, whatever be its method, may be referred, with a strong presumption that the time was ample and that there is no occasion for an evasion of the profound problem of life genesis by referring it to some distant and unknown body; nor is the problem vexed by duress of severe time limits. A theoretical scantiness of time for a prolonged evolution previous to the Cambrian period has been deduced from a molten earth, but this does not apply to the planetesimal hypothesis. The supposed limitation of the sun's thermal endurance would apply if the arguments could be trusted, but their foundation has been cut away by recent discoveries. It is not the least of the virtues of the planetesimal hypothesis that it opens the way to a study of the problem of the genesis and early evolution of life free from the duress of excessive time limits and of other theoretical hamperings, and leaves the solution to be sought untrammeled, except by the conditions inherent in the problem itself, which are surely grave enough.

It is assumed that the conditions on which life is now dependent were prerequisites to its introduction. As already indicated, an

atmosphere and hydrosphere sufficient to sustain life may have been acquired when the earth was about the size of Mars, or one-tenth grown. If, to be conservative, a preliminary growth of twice this amount be allowed, there still remains between this and the Cambrian record the growth of four-fifths of the mass of the earth. So far, therefore, as atmosphere and hydrosphere are concerned, life may have been introduced early in the history of the earth, and may have had a vast interval for development previous to the earliest legible record. There is another essential condition—a sufficiency, but not an excess, of heat and light. If the formation of the parent nebula involved only the outshooting of a small fraction of the ancestral sun, the solar supply of heat and light may not have been so seriously disturbed as to have fatally affected its availability to furnish what was necessary for life at any stage of the earth's growth. The planetesimals between the earth and the sun during the early stages, before they were much swept up by the inner planets, may have screened off some appreciable part of the sun's heat and light, but the ratio of nebular matter to space was probably too small to render this loss critical. So long as the nebula itself remained luminous the nebular light compensated in greater or less degree for the solar light cut off, but perhaps not for the heat. The nebulous surroundings of the growing earth must have somewhat reduced the loss of heat by radiation into space, and so have made some compensation.

There was, however, a terrestrial source of heat and light of critical importance, namely, that arising from the infall of the planetesimals. If this infall were at a rate sufficient to heat the surface of the earth above 100°C., life of the present types would have been prohibited. The present stage of the inquiry does not permit any very confident opinion as to whether this excess was reached or not. Leaving this question open, it is to be noted that if, at the stage when first an atmosphere and hydrosphere could be held, the infall of planetesimals was so rapid as to heat the surface to a prohibitive temperature, the rate of infall must almost certainly have declined as the number of planetesimals in the earth's feeding zone was diminished; so that, long before the supply was exhausted and growth ceased, the rate must inevitably have fallen below the prohibitive limit. If, therefore, the earth were too hot for life when one-fifth grown, its temperature might have become suitably mild when one-fourth, one-third, one-half, or three-fourths grown. Growth after this permissive stage was reached would be slow, and the period required for its completion would still be long.

In the early stages the danger seems to be all on the side of too great heat. Even if the sun's heat were much less than now, the heat of planetesimal infall would probably make up the deficiency and more. The infall would continue to be a source of home supply so long as the accretion continued, declining as the supply of planetesimals diminished. This diminution of the supply cleared the space between the earth and the sun, and gradually brought the sun into full function. There would, therefore, be a gradual passage from the partial dependence on the home supply of heat and light to complete dependence on the solar supply. There is little ground for apprehension that the infalling planetesimals would be seriously dangerous to the early forms of life, for in the first place the atmosphere must have been then, as now, an effective cushion, checking the speed of the planetesimals and partially dissipating them, and, in the second place, the early organisms were probably all aquatic and were further protected by their water covering.

The introduction of organic activity is presumed to have brought into play the well-known attendant chemical processes. The changes in the composition of the atmosphere are especially important. It has been indicated that the primitive atmosphere probably contained a preponderance of carbon dioxide, and a little later carried all the water-vapor it could hold under the prevailing temperatures, while the amount of nitrogen was not improbably low, and that of oxygen uncertain. If only there were oxygen enough to serve the functions of plant life at the outset, the existing large content of oxygen could probably all arise from subsequent plant action. It is merely necessary, therefore, to assume (1) that the carbon dioxide was not too abundant to prohibit the development of the early plants; (2) that the oxygen was sufficient for their vital processes; and (3) that the nitrogen was much less abundant than now, to give a good working basis for the evolution of the present very different atmosphere. Assuming that green (photogenetic) plants were first introduced, and that until some time later there were no animals or predaceous plants which decomposed the carbon compounds produced by the green plants, the first effect of the plant life on the atmosphere would be to reduce its carbon dioxide and increase its free oxygen. If there were no check or offset to this process, a relatively short time would suffice for the conversion of an atmosphere of dominant carbon dioxide to one of dominant oxygen. If the present vegetation can remove the present content of carbon dioxide in 100 years, as estimated, an amount of carbon dioxide as great as the whole

atmosphere of to-day might be changed to oxygen in about 300,000 years by an equally active vegetation. The early plant action may have been much less efficient than that of to-day, and the requisite period might be correspondingly lengthened, but it might still be geologically short. Besides, the early atmosphere, by hypothesis, was much less abundant than the present one and probably much more active in the carbonation of rocks.

It is assumed that life requiring a high content of oxygen did not appear until after the composition of the atmosphere had been suitably changed in this way. After oxygen-consuming, carbon-dioxide-freeing organisms came into existence the reciprocal action of the two classes of life tended to maintain an equilibrium, though not an equality, between the oxygen and the carbon dioxide in the air. At the same time the carbon dioxide was continually uniting with the rock substance of the outer part of the earth, as it does now, and was thus being removed from the atmosphere. The same is true of the oxygen; but probably then, as now, oxidation was less active and prevalent than carbonation, and so the combined result of plant life and of inorganic action was to bring down the content of carbon dioxide to a subordinate place. The nitrogen, being relatively inert, gradually accumulated, and has now become much the most abundant constituent.

So soon as plants and animals had come into action, all the great factors potential in the earth's physical evolution were in play.

By hypothesis, volcanic action only began some time after the beginning of the earth's growth, for it was delayed (1) by the lack of sufficient compression in the central parts to give the requisite heat, and (2) by the time required for this central heat to move out to zones of less pressure, where it would suffice to melt the more fusible constituents. But, once begun, it is supposed to have gradually increased in actual and in relative importance until it reached its climax. This obviously came much later than the climax of growth, for it was dependent on the growth to give the increased compression from which arose the central heat on which the vulcanism depended. And so, owing to the sources of delay just cited, the maximum of volcanic action must have lagged much behind the accession of the material which remotely actuated it. It is therefore inferred that vulcanism continued to increase in activity long after growth had entered on its decline, and that there was an important period in which the dominant activity was volcanic.

It is conceived that in the late stages of the earth's growth the

amount of material poured out on the surface in molten form or introduced into the outer parts of the earth from below was very much greater than the accessions from without. Still later, these declining accessions were so overwhelmed by the igneous extrusions that they became indistinguishable contributions. In this stage, too, it is held that the modifications wrought by the atmosphere, the hydrosphere, and organic life were also quite subordinate to the volcanic contributions. Disintegration is assumed to have gone little farther, usually, than to partially reduce rocks of the granitoid type to arkoses, and those of the basic type to wackes. Rather rarely, it is believed, was much pure quartzose sand, aluminous clay, or similar well-decomposed residuary materials accumulated; rarely, also, much carbonaceous shale. Arkoses and wackes, when metamorphosed later, took on such a similitude to igneous rocks as to be more or less unidentifiable.

The formations of this period of volcanic dominance, with very subordinate clastic accompaniment, are regarded as constituting the Archean complex, though perhaps only the later portions of the great volcanic series are represented by the *known* Archean.

I have studied at considerable length the problem of deformation of the earth under the several hypotheses of its origin and the conditions sequent thereon. The most difficult feature is to bring into working harmony the agencies that produce lateral thrust of the outer crust as demonstrated in the extensive folding and reverse faulting, on the one hand, and the vertical movements exemplified in plateaus and normal faulting on the other. Current views are attended by grave difficulties when an attempt is made to reduce them to quantitative terms. I have developed what appears at present a very promising line of solution, but I prefer to work upon it somewhat further before reporting upon it.

I desire to direct attention to the fact, frequently indicated by allusions in the preceding statement, that further deployment, and particularly further testing of the hypotheses and sub-hypotheses, all along the line, are definitely contemplated. While they have been constructed with some hope that they may be in the line of the ulterior truth, it is felt that their only assured value lies in the aid they may render in the development of tributary investigations, and in assembling and interpreting the varied data from the multitude of sources from which so complex a problem must necessarily make drafts. The accompanying communication of Dr. Moulton indicates in particular that a severe testing of our own hypotheses, as well as those of others, is a part of our working scheme.

In my last report mention is made of the preliminary stages of an inquiry relative to changes in the form of the earth growing out of hypothetical changes in the rate of rotation due to tidal action. The inquiry as originally planned could easily have been carried out, as Professor Slichter had contributed the necessary computations and it only remained for me to add the geological discussion. This would, however, only have introduced a conflict between geological deductions and the well-known tidal deductions of G. H. Darwin. It seemed therefore desirable that the influence of tidal attraction should be recomputed on the assumption of a rigid earth instead of a viscous one, not only, but on the assumption of increasing rigidity toward the center—an assumption that seems to be required by several recent lines of evidence relating to the state of the earth's interior. It seemed also desirable that the assumption should involve high elasticity of form, which seems also to be indicated by the rates of transmission of transverse seismic oscillations through the deeper parts of the earth. I have not as yet been able to arrange for this rather laborious work, owing to the engagement of the available parties competent to undertake it.

I append herewith statements of the collaborative work of Doctors Moulton, Lunn, and Stieglitz, the general nature of which was outlined in my last report.

Respectfully submitted.

T. C. CHAMBERLIN.

CHICAGO, *September 30, 1904.*

---

## THE WORK OF DR. STIEGLITZ.

CHICAGO, *October 26, 1904.*

DEAR PROFESSOR CHAMBERLIN: I beg to report that I have made considerable progress on the problem of possible relation of the deposits of pure gypsum beds, free from calcium carbonate, to the carbon dioxide content of the atmosphere and the climate at the period when the deposits were laid down; but I think it is advisable to pursue the subject further before reporting any specific results.

Yours respectfully,

JULIUS STIEGLITZ.

THE WORK OF DR. MOULTON.

CHICAGO, *September 29, 1904.*

DEAR PROFESSOR CHAMBERLIN: I regret that unforeseen conditions have prevented me from working more than two months on the nebular hypothesis in the last year. The prospects now are that I shall be able to carry out the work of the exhaustive critical review without further serious interruption.

It has seemed to me essential to make a careful preliminary discussion before taking up the work professedly referring directly to the nebular hypothesis. These preliminary discussions are on (*a*) the different kinds of hypotheses and their uses, (*b*) the observational data pertinent to the inquiry, and (*c*) the laws which have been derived from the data. Then will follow the discussion of the work done on the nebular hypothesis. The first epoch reaches up to Laplace, the second consists of Laplace and the commentators on his work, including the modifications introduced by the theory of the conservation of energy; the third starts with Darwin's work on tidal evolution and reaches to our work in 1900.

(*a*) *Different Kinds of Hypotheses and Their Uses.*—In this I have attempted, in the first place, to analyze hypotheses with respect to the character of their origin and relation to observational data. In the second place, I have attempted to form an estimate of the value of these various sorts of hypotheses in scientific work. I am firmly convinced that this work is of value apart from the later discussions, and that it is particularly valuable in connection with the estimates of the work done on the nebular hypothesis by the various writers.

(*b*) *Observational Data.*—This and the next topic are almost universally largely mixed. They are purposely sharply separated here, for the observational data are a permanent acquisition, while the laws are hypotheses derived from them. Since the final theory, the nebular hypothesis, is in question, the preliminary hypotheses or laws can not be passed over lightly.

(*c*) *Scientific Laws.*—This topic and the preceding have led me into every field of physical science. The laws have been (and are being) analyzed on the basis of (*a*) and their probable validity examined. This task of looking critically at the foundations of all laws upon which the nebular hypothesis is based is very heavy.

The preliminary discussions (*a*) and (*b*) are practically complete. The work on (*c*) has made some progress, but it largely remains to be done. Most of the data on the nebular hypothesis have been col-

lected, but the work of carefully comparing them with the results obtained in (*b*) and (*c*) still remains. This will take much time. For example, a thorough review of Darwin's works or of Ritter's can not be done inside of six months. Nearly all of this work is entirely unverified and should be gone over. Besides, I am planning to make every conceivable cross-test on every theory. The work brought out in somewhat separate lines after the publication of many of the original papers makes this a serious task.

Very truly yours,

F. R. MOULTON.

---

## THE WORK OF DR. LUNN.

CHICAGO, *September 30, 1904.*

DEAR PROFESSOR CHAMBERLIN: After making the progress previously reported to you, I was compelled by the strain of other work to lay the geological problem aside almost entirely for several months, but during the past two months it has been constantly before me. I think it will serve the purpose of your report of progress if I set forth the way in which the matter has developed. The sketch may be brief, because at least part of the manuscript will be ready for publication so soon.

Our object was to determine the total amount and distribution of heat due to the gravitational energy resulting from the contraction to its present condition of an earth originally homogeneous and having the density of the present surface rock. It was thought that this would represent fairly the thermal effects that would arise from the formation of the earth by aggregation. There is not the slightest difficulty in determining the total amount of that energy for any assigned law of density; but the question of its localization in the mass antecedent to its transfer, by conduction or by extrusion through volcanic processes, can not be answered without recourse to hypothesis as to the thermodynamic properties of the substance at the high temperatures and pressures met with. The results which I have already furnished you refer to the energy generated by what might be called static compression, each portion of the mass being conceived as heated by local compression from the surface density to its present density, the work done being assumed to produce a proportionate rise of temperature. The form of the original temperature curve corresponding to this and the main features of the

subsequent cooling were determined, on several assumptions as to conductivity and internal density. I have not thought it worth while to regard the specific heat as other than constant, because of the uncertainty attending (1) the application of Fourier's equations at such high temperatures, and (2) the very definition of temperature under these conditions.

This much is practically ready for publication, except the rounding off of the mode of presentation. I think it would hardly pay to attempt more in this direction just now, and I plan to offer this, with a critique of the assumptions, as part 1 of the paper to be sent in shortly.

The energy so generated is, however, not the entire amount of gravitational energy, though perhaps in any ordinary case the major portion. The reason is that—assuming, as we have done, that the pressure depends only on the density—a dynamic equilibrium is possible only in the final state of the mass; consequently the passage from the homogeneous to the compressed condition must be accompanied by the generation, in addition to the strictly compressional energy, of the kinetic energy of a more or less oscillatory motion, which would be transformed to heat by the internal friction due to the viscosity of the mass. The problem of determining how this portion of the energy is localized is a very puzzling one. The exact determination of every phase of this motion is hardly to be expected, since even the analogous problem for a globe of perfect gas leads to equations of whose solution practically nothing is known. However, the features of the solution in certain analogous, though much simpler, cases of damped acoustic and electromagnetic vibrations suggest that the "asymptotic" case for infinite coefficient of viscosity can be made more easily accessible, and this result would probably be useful, since the viscosity of lava is actually so great. I am hopeful of success in this direction, but have nothing complete to offer yet.

The theory needs to be completed in another respect before I can be satisfied with it. The contraction due to cooling makes the generation of heat proceed parallel with its conduction. On account of the small coefficient of expansion, the heat thus added is negligible in a small body, but becomes an important portion of the whole in a mass as large as the earth. Hence to follow the process strictly it would be necessary to consider the conduction and contraction as simultaneous, following the initial compression. The difficulties in the way here are serious.

I have therefore begun a search for assumptions as to the thermodynamic properties of lava, which would be consistent with the data at hand relative to surface rock, and to follow out the plan carried through by Ritter for gases and vapors, determining the "adiabatic condition line" and the law of contraction and radiation, assuming the adiabatic state maintained by an appropriate law of conduction. If the law of conduction so determined should prove plausible, and the rate of surface loss agree fairly well with observation, this would furnish a complete solution of a case perhaps not remote from the actual, and the direction of departure from it would give at least qualitative information of value. This is the only avenue of approach I see just now. It is impossible to satisfy the conditions by assuming a coefficient of expansion so small that even for the earth the cooling is practically independent of the contraction, for the thermodynamic law of entropy shows that part of the energy from the gravitational source must take the form of internal *potential* energy, not temperature, and the smaller the coefficient of expansion, the larger this portion is. For a fictitious substance with zero coefficient of expansion, there would be no rise of temperature at all. This striking result from the law of reciprocity only shows again what a great difference there is between the small masses of the laboratory and the cosmic masses.

It is fair to say that these criticisms are not peculiar to our point of view, but apply with equal force to everything which I have seen on the secular cooling of the earth.

Very truly yours,

ARTHUR C. LUNN.

# PLANS FOR OBTAINING SUBTERRANEAN TEMPERATURES.

On November 19, 1902, I submitted to the Trustees of the Carnegie Institution a memorial proposing an investigation of subterranean temperature gradient by means of a deep boring in plutonic rock. On December 11, 1903, I was notified that an appropriation of $1,000 had been made by the Institution for the expense of preliminary work and the preparation of plans, and was requested to take general charge of the preparations of plans.

I now have the honor to submit the following report of progress:

(1) Mr. F. H. Newell, Hydrographer of the United States Geological Survey, has at my request considered the question of cost, securing from establishments engaged in the manufacture of well-drilling machinery estimates of the expense of putting down borings to great depths. These estimates indicate that the cost of a boring in granite to the depth of 10,000 feet would be very large—so large as to be prohibitory. The Sullivan Machinery Company estimates the cost of a boring to the depth of 6,000 feet at $110,000, and is willing to enter into a contract on the basis of that estimate. Estimate for a 6,000-foot boring has been requested from another responsible company, but has not yet been received. If the general plan is approved by the Institution, bids will be solicited from parties making a business of sinking wells under contract.

(2) I have investigated the question of a suitable site (*a*) by formulating the conditions to be satisfied, (*b*) by a series of inquiries and consultations with geologists familiar with the structure of various districts east of the Great Plains, (*c*) by a personal visit to the district which appeared from description most likely to afford a satisfactory site. As a result of this investigation I beg to report that the Lithonia district, Georgia, both appears preferable to all other districts of which I have secured information and does in fact well satisfy the conditions requisite for a successful boring. No effort was made to choose a precise spot, but the natural conditions are there favorable over so large an area that the selection of a particular spot can be made in view of local economic conditions.

(3) By favor of the Director of the United States Geological Survey, the coöperation of Mr. Newell and other members of the Survey has been secured without expense to the Carnegie Institution, and the only draft thus far made on the allotted fund has been for the

expense of my trip to Georgia—$80.69. It is anticipated that further draft will be made when plans for boring have been so fully developed that they may be advantageously submitted to an engineering expert.

(4) In view of the fact that a site has been found at which the essential natural conditions are realized, and of the further fact that an experienced and responsible well-boring company has such confidence in the feasibility of a 6,000-foot hole as to be willing to guarantee its completion, I recommend that the making of a deep boring be undertaken by the Carnegie Institution.

(5) I recommend further that the sum of $65,000 be allotted, of which $10,000 be available in the calendar year 1905, and $27,500 in each of the two succeeding years. This recommendation does not imply the adoption of the contract plan, the question of business method being left open.

(6) I recommend that the control and supervision of the work be intrusted to a committee of three persons, one of whom shall be a physicist, one a geologist, and one a man practically familiar with boring operations.

(7) I submit herewith a discussion of the value to science of the proposed boring, of the considerations affecting the determination of a suitable site, and of the local conditions of the Lithonia district.

Respectfully submitted.

G. K. Gilbert.

Washington, D. C., *September 28, 1904.*

## VALUE AND FEASIBILITY OF A DETERMINATION OF SUBTERRANEAN TEMPERATURE GRADIENT BY MEANS OF A DEEP BORING.

By G. K. Gilbert.

### SCIENTIFIC NEED OF KNOWLEDGE OF THE NORMAL GRADIENT.

Theories of the origin of the earth are intimately related to theories of the constitution and condition of its interior. In the field of geophysics there is probably no problem which does not involve the distribution of internal heat. Direct observation of the nucleus being impossible, inference is depended on, and inferences, so far as they are quantitative, have been and perhaps can be based only on observation of temperature gradient near the surface. For the purpose of testing theories as to the origin of internal heat, it is important to know not only the temperature gradient in the accessible portion of the crust, but also the variation of gradient with depth. If the relations of crust to nucleus have existed so long that the distribution of heat has become systematic, and the heat discharged at the surface is derived from all parts of the sphere, then the gradient in the accessible zone near the surface should be sensibly uniform. If the heat flowing toward the surface is and has been derived from tidal work performed in a subcrustal zone, then also the observed gradient near the surface should be uniform. But if, as assumed by Kelvin and King, the heat of the earth received its general distribution through convection during an initial molten condition, and surface cooling has been in progress only a few million years, then the gradient in the upper portion of the crust should diminish downward.

### NEED OF A NEW DETERMINATION.

Temperature gradients observed in mines and in wells and other borings present a wide range, and the mean derived from them would probably be found to have a large probable error. But even if its probable error were small, the mean could not claim high precision, because most of the observations heretofore made have been subject to unfavorable conditions. Deep mines exist because of geologic disturbances involving either volcanism or diastrophism, and in either case calculated to disturb for a long time the normal distribution of heat. They exist also because of lack of uniformity of the rocks, and in varied rocks there are usually variations of gradient dependent on variations of conductivity. So there is

always a presumption that the gradients observed in mines are abnormal or abnormally varied. Artesian wells are made in order to utilize the subterranean circulation of water, and that circulation involves the convection of heat, whereby the normal gradient is necessarily disturbed. Oil-wells and gas-wells can be successful only in regions where the strata encountered at different depths are of diverse character, and the temperature gradient theoretically changes in passing from rock of one character to rock of another. The successful wells have their normal temperatures disturbed by the expansion of gas; the unsuccessful usually penetrate zones of water circulation. As these three categories include practically all the deep openings which have been made in the earth, it is evident that the combination of their data yields no trustworthy index of the normal downward increase of temperature. The arithmetic mean of all their results has less authority than a single determination made under proper conditions. The ideal determination is to be obtained by boring in homogeneous rock not recently subject to disturbances calculated to modify its heat distribution. And such rock will not be exploited in intelligent search for any economic material. The determination which shall be of service to the student of geophysics must be made by a boring planned and executed for the special purpose.

Special emphasis may be given to the fact that all deep mines and all deep borings heretofore made have penetrated varying instead of uniform material. They have, therefore, presumptively encountered changes in temperature gradient arising from differences in material, and as it is not practicable to separate such variations of gradient from the variations dependent merely on depth, the latter variations can not be deduced from records now in existence. They can be afforded only by a deep boring in homogeneous material.

## CONDITIONS TO BE SATISFIED IN THE SELECTION OF A SITE FOR A BORING.

*Uniformity of Rock Character.*—The temperature gradient within the earth's crust, or the temperature change per unit of vertical distance, varies locally with the conductivity of the material (more strictly, with the diffusivity, which is a function of the conductivity and the specific heat). It may be subject also to other variation, but the discussion of other sources of variation is practically impossible if their effects are complicated with those arising from diversity of rock character. It is conceivable that the thermal record of

a boring traversing a series of diverse rocks might be corrected for the conductivities of the several rocks, but the determination of subterranean conductivities is a matter of such difficulty that a trustworthy correction can not be applied, and the difficulty can be met only by avoiding the necessity for correction. The first condition, therefore, to be satisfied in the selection of a locality for a boring is that the rock be of uniform character for the whole depth of the boring.

*Continuity of Rock.*—The disturbing factor which impairs most records of subterranean temperature is subterranean circulation of water. There are few districts of sedimentary rock exempt from subterranean circulation. Descending currents entering regions of higher temperature receive heat from the rocks they traverse, and this heat is carried to the surface by ascending currents. Thus convection partly replaces conduction as a conveyor of heat, and the conditions are rendered unfavorable for the development of the normal temperature gradient. As circulation is promoted by all cracks and other partings of the rock, as well as by porosity, it is important that districts where these occur be avoided. The second condition to be satisfied in the selection of a site for a boring is that the rock be continuous, or massive, and impervious.

*Topography of the Surface.*—Every modification of the earth's surface causes a modification of the subjacent isogeotherms, and if the change is rapid it causes a temporary irregularity in the isogeotherms near the surface. If the result of the topographic change is a plain, the isogeotherms eventually become parallel planes with regular intervals; but if the result of the topographic change is a surface of bold relief, the isogeotherms tend toward an adjusted distribution which reflects the topographic irregularities.

From these considerations arise two conditions to be taken into account in the selection of a place for boring. It should be a plain or surface of low relief, and the plain should be one which has not received a heavy deposit during the later geologic periods.

*Stability of Surface Condition.*—The temperature of the surface of the ground is ordinarily determined by the mean annual temperature of the air. The temperature of the bed of the ocean is similarly determined by the temperature of the water; and the temperature beneath a glacier is determined by the basal temperature of the ice, which is approximately 0° C. These surface temperatures are the initial or control temperatures to which the isogeotherms conform. If they are changed, a readjustment of isogeotherms is at once instituted, and during the period of readjustment the spacing of isogeotherms, or

the arrangement of gradients, is abnormal. Usually the temperature of a coastal plain is not the same as the temperature of the adjacent sea bottom, so that a submergence or an emergence of a locality creates a disturbance of isogeotherms. Similarly, the creation of an ice-sheet and its removal cause changes of the surface temperature and derangements of the isogeotherms. Outside the regions of actual Pleistocene glaciation there were Pleistocene changes of climate, by which the isogeotherms must have been deranged. These changes were probably greatest in high latitudes and less in low latitudes. The resulting conditions to be satisfied in the selection of a site for boring are: (1) That it shall have experienced no change in later geologic periods from marine to land conditions, (2) that it shall not have been covered by Pleistocene glaciers, and (3) that it be in low latitude rather than high.

*Relation to Volcanism.*—The movements of lavas, their intrusion among other rocks, and their extrusion at the surface effect great changes in the distribution of subterranean heat, and create disturbances in the regularity of isogeotherms which are very slowly effaced. The resulting condition for the selection of a site is that it be not near a locus of volcanism in any of the later geologic periods.

*Relation to Diastrophism.*—Orogenic disturbances, or those resulting in the flexure and faulting of rocks, not only stimulate subterranean circulation, but produce local concentrations of heat as the product of mechanical and chemical work. The thermal irregularities thus instituted disappear very slowly. The resulting condition for the selection of a site is that it be in a region not subject to orogenic disturbance in any of the later geologic periods.

## THE SELECTION OF A LOCALITY.

In the practical search for a locality suited for the proposed deep boring it seemed proper to restrict attention to the territory of the United States, and in the application of the criteria enumerated above I soon reduced the field of inquiry to narrow limits. The condition that the rock penetrated should be of uniform composition and of massive character barred all regions occupied by sedimentary formations, for these are everywhere more or less heterogeneous, and in nearly all localities admit the passage of circulating waters. The only large bodies of rock whose uniformity is reasonably assured are plutonic, and attention was therefore limited to the large batholiths.

In the Cordilleran region most of the mountain ranges are young and are unfitted for the purpose, both because the temperature dis-

turbances created by their uplift can not be assumed to have disappeared and because their topographic ruggedness implies irregularity of isogeotherms. New England and the region of the Great Lakes are unfitted because they were covered by the Pleistocene ice-sheet. Attention was therefore restricted to the batholiths of the Southern States.

As to these I sought information from my colleagues on the Geological Survey, finding the available information so full that I was able to exclude some because associated with bold topography, others because lacking uniformity of composition, and others because traversed by joints. Of the localities not thus excluded the most favorable appeared to be the Lithonia granite district in Georgia. Of this I made a personal examination, and as it seemed peculiarly favorable to the purpose, no other examinations were made.

### THE LITHONIA DISTRICT.

In its general topographic character the Lithonia district is a plain. The stream valleys, for the most part open, are excavated to depths of 50 to 150 feet. A few rounded bosses of granite project from 50 to 150 feet above the plain. The granite is surrounded and in part overlain by schists, which appear to have originally constituted the walls and cover of the batholithic chamber. The continuity of the granite mass from outcrop to outcrop is inferred from the close lithologic similarity found at all the outcrops. This similarity includes not only composition, but a peculiar and unusual structure, the granite having an imperfect schistosity, the planes of which are everywhere contorted. It is therefore called by the State Geological Survey *contorted granite-gneiss*. The rock is massive. Only a few joints were observed, and these appeared to be occupied by thin veins, and thereby sealed, so as not to affect materially the continuity of the rock. The partings utilized in quarrying are parallel to the surface and are usually not natural, but created by blasting. They indicate a tendency toward exfoliation, which is one of the characters of massive granite. In recent studies in the Sierra Nevada I have found the tendency to develop partings parallel to the surface characteristic of massive rocks and absent from rocks traversed by systems of joints.

The extent of the granite body is not less than 10 miles in one direction by 3 miles or more in the transverse direction. Uniformity of character through such an area affords reasonable presumption that uniformity will be found in the vertical direction to such

depths as are obtainable by the driller. The age of the batholith is not definitely known, but it is believed by students of Georgia geology to be probably pre-Paleozoic, and certainly not later than early Paleozoic.* Of the later geologic history all that is demonstrated by the features of the locality is profound degradation, resulting in the development of a broad peneplain. Nothing is known in the vicinity of later orogenic or volcanic events, and the Cretaceous and Tertiary formations of the Coastal Plain are thought not to have covered this area. So far as is known, the region is one characterized by prolonged geologic quiet, and it has probably been exempt, as far as any locality which might have been selected in the United States, from physical and climatic accidents competent to disturb the arrangement of subterranean temperatures.

*Economic Conditions.*—While the selection of the Lithonia district for the proposed boring was made solely on considerations arising from the scientific demands, attention was also given while on the ground to economic considerations affecting the cost of the work. One of the essentials in the use of the diamond-drill is a good supply of water. This can be readily secured in the final selection of the precise site of the boring. The district is crossed by a railroad, from which several spurs run to quarries, and a suitable site can be found near one of these lines. No serious problems are connected with the transportation of machinery and fuel. There is rail communication with the neighboring city of Atlanta.

## ACCESSORY INVESTIGATIONS.

In the planning of the boring no other instrument has been considered than the diamond-drill. The rock could probably be penetrated by the churn-drill at less cost, but the churn-drill, by grinding the rock to sand, destroys its structure and makes it impossible to be assured of the uniformity of its lithologic character. The diamond drill, on the other hand, removing part of the rock in the form of a core, preserves a continuous record of the character of rock traversed. The core, moreover, permits the prosecution of other investigations in addition to the thermal. The strength and other physical properties of deeply buried granite are practically unknown, and the information which can be obtained as to these may prove of importance to geophysics.

It is at least worthy of suggestion that the boring could also be utilized for the subterranean swinging of a specially constructed

* Geol. Survey of Georgia, Bull. No. 9 A, 1902, p. 63.

pendulum, and the measurement of the earth's weight by means of a vertical pair of gravity determinations could thus be repeated. The homogeneity of the crust layer between the upper and lower stations and the representative character of the rock samples brought up as drill cores would be peculiarly favorable for the determination of the density of the crust layer.

To give high precision to the determination of density it would be necessary to take account of the compression of the rock under stress of the superincumbent weight. Rock compression has not yet been measured in the laboratory, the matter being one of extreme difficulty, by reason of the deformation of both samples and testing apparatus when great pressures are applied; but there is reason to think that valuable observations bearing on this point could be made within the boring at some stage of the work. It should be possible, by suitable automatic appliances, to measure that resilient elongation of the column of rock constituting a section of core which theoretically takes place while the drill is separating it from the general mass. The importance to geophysics of experimental determinations of rock compression is generally recognized.

# PROPOSED MAGNETIC SURVEY OF THE NORTH PACIFIC OCEAN.

By L. A. Bauer and G. W. Littlehales.

October 3, 1904.

I beg to submit herewith a project for a magnetic survey of the North Pacific, by Messrs. L. A. Bauer and G. W. Littlehales. Reference to this project was made in my letter of the 29th ultimo, requesting that the grant to the Department of International Research in Terrestrial Magnetism for the next year be $25,000. It will be noticed that the project does not call for a separate grant, but is instead a proposal as to the direction in which field work of the department could profitably and advantageously be taken up next year.

Accompanying the project will be found letters from Captain Creak, formerly superintendent of the compass department of the British Admiralty, now retired, and from Superintendent Tittmann. Captain Creak took an important part in designing the British Antarctic ship *The Discovery* and in planning its magnetic work.

Very respectfully,

L. A. Bauer, *Director.*

---

While the state of our knowledge of the distribution of the earth's magnetic forces over oceanic areas, owing to the paucity of precise data, is in general exceedingly unsatisfactory, this is especially true for that great body of water the Pacific Ocean, rapidly developing in great commercial importance.

Except for data from occasional expeditions and such as were acquired in wooden vessels a long time ago, the present magnetic charts used by the navigator over this region depend largely upon the observations on islands and along the coasts. Such land observations, however, are rarely representative of the true values, because of prevalent local disturbances. It is therefore impossible to make any statement as to the correctness of the present charts.

The demands of science, as well as those of commerce and navigation, require a systematic magnetic survey of this region under the most favorable conditions possible, and that the work be done

under the auspices of some recognized research institution in order to insure that the scientific aspects of the work receive their adequate recognition.

It is believed that it will be best to undertake first a magnetic survey of the North Pacific Ocean, and a project is here accordingly outlined which, upon careful consideration and solicitation of expert opinion, is believed to be thoroughly feasible. The project permits of useful comprehensive results being immediately obtained, and is one which can be interrupted without any important waste of antecedent expense whenever circumstances may render a discontinuance or a modification of the original plan advisable. Upon the completion of the survey of this region, which, in accordance with the plans, will not require more than three years, the survey of other oceanic areas may usefully be considered.

The plan is, in brief, to charter a wood-built, non-magnetic sailing vessel of about 600 tons displacement, which, starting out in summer from San Francisco, shall pursue a clockwise spiral course embracing the entire North Pacific Ocean, as shown in red ink on the submitted Pilot Chart. The object of planning such a course is to gain continuous advantage throughout the survey of the dynamical agencies of the atmosphere and the ocean, in passing in succession into each of the five-degree quadrangles into which the chart is divided and in which observed values of the three magnetic elements need to be obtained.

The seasonal shifting of the permanent centers of barometric pressure will cause a variation from month to month of the conditions of wind and current that are represented on this particular chart; but if the departure from San Francisco be taken in the summer the chain of meteorological events will contribute toward the maximum progress over the course, passing thence along the west coast of America to the vicinity of the Galapagos Islands; thence across the Pacific, in latitude between two and three degrees north; thence along the eastern side of the Philippine Archipelago and the Empire of Japan; thence eastward in about latitude fifty-two degrees north; thence to the latitude of San Francisco, and thence continuing through the series of areas bounded by parallels of latitude and meridians of longitude, each five degrees apart, lying next on the mid-ocean side of the circuit last made, and proceeding gradually and by successive circuits into the central region of the North Pacific.

The total length of the course marked out is about 70,000 knots. However, as will be noticed, each of the first circuits practically closes

at San Francisco ; so that, if it is found that the method pursued is not the best, the work can readily be terminated or modified. Each circuit is so planned as to contribute the maximum results with the highest efficiency.

From letters received in response to inquiry (two of the letters are appended) it would appear that the entire work of observation and reduction can be accomplished in three years. The cost per month of the field work, inclusive of all expenses and services, will approximate $1,500. Counting eight months of continuous service per annum, the total annual outlay would be about $12,000. This sum can be provided for out of the allotments for field-work available to the Department of International Research in Terrestrial Magnetism if the annual grant to this department be made $25,000, as per the original plan published in Year Book No. 2.

The region it is proposed to survey fortunately contains magnetic observatories in requisite number and proper distribution for furnishing the necessary corrections to the observed magnetic elements to reduce them to a common epoch. Thus, continuous records of the magnetic variations required for this purpose will be available from the following stations : Sitka (Alaska), Honolulu (Hawaiian Islands), Manila (Philippines), Shanghai (China), Tokio (Japan). In addition to these, it is possible that there may be at the time of the proposed magnetic survey magnetic observatories in the Samoan Islands, in Siberia, and in California or vicinity in position to lend effective coöperation.

Furthermore, the numerous ports and islands will furnish excellent opportunities for controlling instrumental constants and for obtaining any additional variation data that may be needed.

It should also be pointed out that the plan of the courses as mapped permits ready adjustment for closed areas of the observed quantities in accordance with the potential hypothesis, and it may even permit the testing of the accuracy of this assumption, though as regards the latter more can be said at the end of a year's work.

While it is not anticipated that any marked irregularities in the distribution of the earth's magnetism will manifest themselves over the deep waters of the Pacific, it may be confidently expected that in the neighborhood of the islands and along the coasts distortions and irregularities will reveal themselves. With the aid of the results of the detailed magnetic survey of the United States and Alaska, opportunity will therefore be afforded of studying the effect of the configuration of land and water upon the distribution of the

magnetic forces. The first circuit, passing as it does along the American and Asiatic coasts, will yield especially interesting results in this respect. Thus, for example, along the Aleutian Islands marked local disturbances will be revealed. Reports are received frequently from mariners in this region regarding the unsatisfactory behavior of the compass; it is therefore greatly to be desired that a magnetic survey of the waters in this region be made with all necessary detail.

The letters appended will give further information regarding the plan, and will give evidence of the opinions held by those competent to judge.

---

[*Letter from Capt. E. W. Creak to Dr. Bauer.*]

9 HERVEY ROAD, BLACKHEATH, LONDON, S. E.,

*August 31, 1904.*

MY DEAR DR. BAUER: The North Pacific Ocean is, with the exception of the voyage of the *Challenger*, nearly a blank as regards magnetic observations, and I therefore think the magnetic survey you propose will be of great value.

In view of a sailing ship being employed, the route marked out in the letter (of which you have sent me a copy) is, I think, well thought out as regards winds, but I would, if I could, have a larger ship than the one proposed, of 600 tons. However, all can be done in a vessel of 600 tons, if of the proper form—a fast clipper is not wanted, but rather a good, wholesome, steady ship in a seaway.

There is one point which I may have mentioned once before, but will bear repetition. The position selected for the magnetic instruments should be entirely free, if possible, from any vertical force in the ship. This especially applies to a sailing ship, which under action of the sails is liable to a constantly varying angle of inclination, and where the vertical force of the ship causes a constantly varying heeling error in the magnetic instruments.

The absence of any vertical force in the ship renders the observation taken on board free from any reference to the shore as regards declination and inclination, the effects of horizontal disturbance, if of moderate amount, being easily accounted for by swinging at sea as opportunity affords.

Lastly, as to a similar close examination to that proposed for the North Pacific being subsequently carried out in the South Pacific,

I fully concur. I have evidence that the large secular change in the magnetic declination which has been going on for the last sixty years in the ocean area between New Zealand and Cape Horn (south of 30° S.) is still in progress and wants far more attention than has hitherto been accorded to it.

Yours very truly,

ETTRICK W. CREAK.

[Formerly superintendent of the compass department, British Admiralty. Now retired.]

---

[*Letter from O. H. Tittmann, Superintendent U. S. Coast and Geodetic Survey, to Dr. Bauer.*]

WASHINGTON, *October 1, 1904.*

Dr. L. A. BAUER,

Director Department of International Research in
Terrestrial Magnetism, Carnegie Institution.

DEAR SIR: Your note, submitting a plan for a magnetic survey of the North Pacific, together with letters from Mr. Littlehales and Captain Creak, is before me.

There is no doubt in my mind that a survey for that purpose would result in obtaining data of great and permanent value, and that it should be undertaken.

You have pointed out that the scheme of traversing the Pacific by a spiral route is one that can be interrupted at any time. Valuable results are sure to be obtained in even a partial circuit, and therefore there is no danger of waste of funds through failure.

My own estimate of the time required to cover the field in the manner proposed is three years.

Yours truly,

O. H. TITTMANN.

# GEOLOGICAL RESEARCH IN EASTERN ASIA.

By Bailey Willis.

Under Grant No. 72 and its continuation, No. 116, plans for geological research in eastern Asia were perfected and carried to completion during 1903–1904. The original suggestions for this research were made by Mr. Walcott, with a special view to the investigation of Cambrian faunas and search for fossils in pre-Cambrian rocks in localities which were indicated by the work of Baron von Richthofen. The research was not, however, limited to this specific object, but was stated to have for its broader purpose the comparative study of the geology of North America and Asia. In its execution the special investigation of the Cambrian faunas was given precedence, but work was extended to other branches of the science in the effort to accomplish the more general result in comparative geology. Mr. Arthur C. Spencer, to whom the grant was originally intrusted, was unable for personal reasons to carry out its provisions, and I was authorized in the spring of 1903 to proceed with the investigation. I selected as my associates Mr. Eliot Blackwelder, paleontologist, and later Mr. R. H. Sargent, topographer. Mr. Blackwelder and I left the United States in July, 1903, and, proceeding by way of Europe and the Siberian Railway, arrived at Peking September 20. The months of October and November were spent in making topographical and geological surveys in the province of Shantung, in areas selected on account of the extensive exposures of fossiliferous Cambrian strata. Upon our return to Tientsin in December, we were joined by Mr. Sargent. During January and February surveys for topography and geology were executed along a route 250 miles in length, from Pao-ting fu, in the province of Chihli, westward to the Wutai-shan, the highest mountains in northern Shansi, and thence southward to Tai-yuan fu. The greater part of March was employed in perfecting the work accomplished and in a journey of eighteen days from Tai-yuan fu, Shansi, to Hsi-an fu, Shensi. As this journey was necessarily made by a route previously traversed by Baron von Richthofen and other travelers, no surveys were made beyond the general observations consistent with rapid progress. From Hsi-an fu the party surveyed a route, which in great part had not previously been followed by foreigners, southward across the mountainous region which extends to and beyond the Yangtse. This part of the journey falls into

three sections: (1) the crossing of the Ch'in-ling Mountains on foot, (2) the trip by boat down the Han river from Shih-chuan hsien to Hsing-an fu, and (3) the passing of the mountains between the Han and the Yangtse. The work was greatly delayed by continuous rains and high water during the first three weeks of April, but the party arrived at Wushan, on the Yangtse River, on June 6 and closed its field operations at Ichang, the head of steamboat navigation, on June 8. At Shanghai the party disbanded on June 20, the Chinese interpreter and servants, who had rendered loyal service during nine months, returning to Tientsin, while the three American members took passage for the United States.

The success of the expedition is in large measure due to the assistance which it received on all hands from those who, privately or officially, were in a position to promote its objects. The ministers at Washington, of China, Great Britain, France, Germany, and Russia, and the American ministers abroad, at the respective capitals of these nations, gave the expedition their cordial indorsement. Mr. E. H. Conger, the American minister at Peking, rendered special service in introducing the purpose of the Carnegie Institution to the Imperial government, and in securing for the members of the expedition that official recognition which was essential to safety in the prosecution of surveys in the interior of China. Their excellencies, Yuan Shih Kai, viceroy of Chihli; Chou Fu, governor of Shantung; Chang Tsen Yang, governor of Shansi; and Sheng Fan, governor of Shensi, exhibited an intelligent and broad-minded appreciation of the purpose to advance knowledge, and substantial aid was rendered by many magistrates with whom the scientists came in contact. Pleasant relations were consistently maintained with the many Chinese who gathered from every village to watch the strange operations of surveying, and it is gratifying to record that at no time was there any dispute or difficulty with the natives.

I wish here particularly to express my appreciation of the service rendered science by my associates, Mr. Blackwelder and Mr. Sargent, through their unflagging zeal and earnest scientific purpose; their cordial coöperation at every step of the expedition and their self-restraint in dealing with the natural, but sometimes trying, curiosity of the natives contributed vitally to our success.

Through the courtesy of the U. S. Geological Survey, a plane-table, a telescopic alidade, a large camera, and accessory instruments were supplied without cost. A theodolite, the need of which was not appreciated in the initial plans, was loaned by Col. A. W. S. Wingate, of the British intelligence office at Tientsin.

Subsistence in China includes two distinct causes of expense—that food which you provide and that which is provided for you. The party was supplied with staple articles of foreign food—flour, sugar, coffee, and a small amount of canned goods. In addition to these,

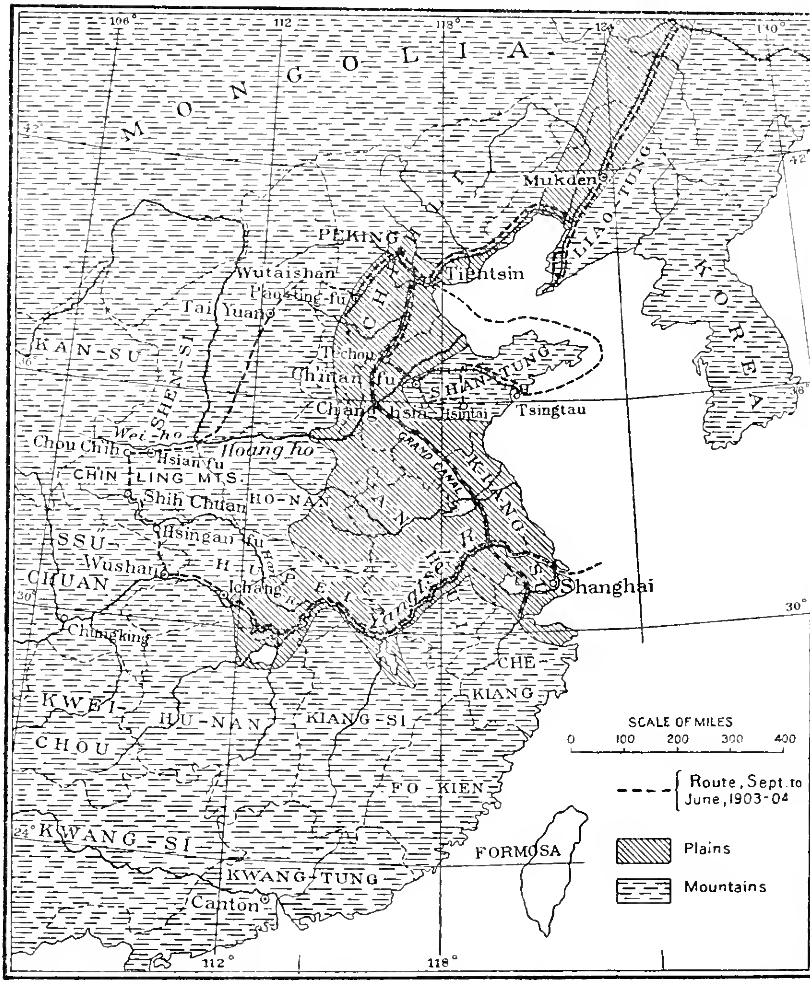



FIG. 6.—Route in Eastern China, September to June, 1903–1904.

fresh meats and vegetables were purchased en route. Though thus prepared to live on its own resources, the party was often furnished with food and lodging at official cost, and for this it was proper to pay an amount estimated equivalent to the service rendered. Wine,

cigars, and candied fruits were taken as gifts to Chinese officials and for their entertainment. A proper use of these articles in return for courtesies received accords with the custom of the country.

The cost of traveling in China has rarely been stated in such a manner as to afford a basis for estimate, and it is desirable to place on record the experience of the expedition for the benefit of those who may plan similar journeys.

A list of daily expenses or rates of expense follows:

*Services:*

Head servant; takes charge of other servants; guarantees their service to you and your pay to them; makes contracts; purchases supplies; may manage official ceremonies and interpret, etc. Per month, $40 to $50.

No. 1 "boy"; acts as substitute for head servant; superintends arrangements at inns; does the work of a valet; takes care of property en route and at inns, etc. Per month, $12 to $16.

Cook; skilled in preparing European food. Per month, $12 to $16.

"Boys" and first-class coolies, employed as personal attendants to perform any required service or labor. Per month, $5.50 to $7.50.

Kumshaws—*i. e.*, gifts—which custom requires in return for temporary service rendered by magistrates' servants, soldiers, and couriers, may be reckoned at rates of coolie pay, from 250 to 400 cash for each servant per day. In case the number of recipients is large and the service slight (an escort of welcome or farewell or the squad of retainers in attendance at a kung kuan during the noon stop), 150 to 200 cash apiece (sixteen cash to one cent gold). When traveling by the highway this charge varies from $2.50 to $4 per day; in country districts away from officialdom it is much less. In case the service is continued some days or longer, there should be added a reasonable allowance for subsistence, including that of a horse in case of a mounted man.

*Subsistence:*

In the selection of foreign food to be carried on the journey individual taste will rule, but the temptation is to carry more than is needed. Coffee, sugar, baking-powder, salt, canned milk, and butter are the principal articles of food not obtainable except in the largest cities of the interior and the ports of China. A ration, including these articles, with flour, lard, dried fruits, sweet chocolate, preserves, and small quantity of canned goods, may be reckoned at 35 cents per man per day. Twenty-five cents additional may be allowed for fresh meats, vegetables, and fruits.

Fuel is usually limited to charcoal and kerosene, but these are both available everywhere except in remote mountain regions. Asphyxiation by charcoal gases is a not uncommon occurrence, on account of the general use of open braziers and the absence of ventilation. An outfit of oil-stoves and portable lamps is to be recommended for winter journeys.

Chinese inns furnish rooms only; all else is extra. The charge for a No. 1 room, with quarters for servants and cooking, is 50 to 75 cents per day.

*Transportation:*

Where horses, mules, or donkeys and their drivers are employed with carts, riding saddles, or pack saddles, 1 tael, about 65 cents, per day per animal is a usual charge.

Wheelbarrow coolies in Shantung; load, 150 to 200 pounds. Per day, ½ tael.

Coolies who carry on their backs, on poles, or in "chairs"; load, 60 to 85 pounds. Per day, ⅓ tael.

Boats: River sampan, 3 boatmen and 1 passenger with baggage. Per day, 2 taels. Houseboat, 5 boatmen and several passengers. Per day, 6 taels.

In all cases those employed "find" themselves, but a "kumshaw" is expected at the end of the service.

The members of the Carnegie expedition were received by the Chinese government as scholars representing a great institution of learning, and Chinese official conditions of living were thus imposed and complied with.

With reference to safety and success, the selection of an interpreter was of first importance. The employment of a Chinese of mandarin rank was strongly urged by residents familiar with official life, but the conditions of travel for geological research were such as to make this arrangement difficult. The suggestion of an official to interpret was put aside, and a "boy," Li-san, was secured upon recommendation of Mr. W. S. Emens, of Tientsin, formerly judge of the criminal court under the provisional government of 1900. Li had been a detective under Mr. Emens, and afterward head boy to Generals Chaffee and Howard. He served throughout the expedition with rare zeal, ability, and honesty, proving himself equally competent in the daily exigencies of travel, in establishing favorable relations with the country people, and as master of ceremonies and interpreter during official visits. To his loyalty, tact, and efficiency much of the success of the expedition is due.

## SUMMARY OF OPERATIONS.

### PREPARATORY.

March 20, 1903. Letter of authority to proceed under Grant No. 72.

May, June, July, 1903. Preparations: Examination of literature, purchase of equipment, establishment of diplomatic relations.

July 27. Messrs. Willis and Blackwelder sailed for England.

August 5 to September 5. In London, Paris, Berlin, Vienna, and St. Petersburg, completing diplomatic relations and in conference with German and Russian scientists in regard to Asiatic problems.

September 6 to 20. En route via Siberian Railway to Peking, with one day at Tomsk for conference with Prof. A. W. Obrutchoff.

September 21 to October 8. In Peking and Tientsin preparing for journey in Shantung.

### FIRST TRIP, SHANTUNG.

October 9 to 12. En route on Grand Canal by launch, Tientsin to Techou.

October 12 to 18. By cart, Techou to Ch'inan fu, 3 days, and 3 days at Ch'inan fu in conference with officials.

Oct. 19 to Nov. 4. Making topographical and geological survey of the Ch'ang-hsia district for stratigraphy and paleontology of the Cambrian strata.

Nov. 5 and 6. En route Ch'ang-hsia to Tai-an fu by horse and wheelbarrows.

November 7 to 12. Geological reconnoissance of the Tai-shan pre-Cambrian rocks and physiographic relations of the mountain.

November 13 to 15. En route Tai-an fu to Hsin-tai by horse and wheelbarrows.

November 16 to December 1. Making topographical and geological survey of the Hsin-tai district for stratigraphy and paleontology of the Cambrian strata and the relations of post-Carboniferous normal faulting to the present topography.

December 1 to 4. En route with geological reconnoissance, Yen-chuang to Ch'en-ts'un, on foot, with wheelbarrows.

December 5 to 11. En route by rail and steamer, via Tsing tau, to Tientsin.

INTERIM.

December 12 to 31. At Tientsin and Peking, packing Shantung collections and preparing for winter trip. Joined by Mr. R. H. Sargent, topographer. Mr. Blackwelder made reconnoissance in Manchuria, December 18 to 30. Mr. Willis made survey of artesian water conditions about Peking, December 22 to 28, at request of the American minister.

SECOND TRIP, WUTAI-SHAN.

(From Peking, via Pao-t'ing fu, Fou-ping, Wutai-shan, and Hsin-chou, to Tai-yu-an fu, Shansi.)

January 1 and 2. Peking to Pao-t'ing fu by rail.

January 3 to March 7. On foot with pack-mules, Pao-t'ing fu to Tai-yuan fu; topographical and geological survey along route, covering a strip five to twenty miles wide and two hundred and fifty miles long, with geology of the pre-Cambrian, Cambrian, and Carboniferous rocks, physiography of the Shansi Mountains, and occurrence of the loess.

March 8 to 12. At Tai-yuan fu, in conference with provincial authorities.

INTERIM.

March 13 to April 1. En route Tai-yuan fu, Shansi to Hsi-an fu, Shensi, by cart, via the highway, with observations on geology, physiography, and loess.

April 2 to 11. At Hsi-an fu in conference with provincial authorities and preparing for journey across the mountains to the Yangtse River.

THIRD TRIP, SOUTHERN SHENSI.

(From Hsi-an fu via Chou chih hsien, Shih ch'uan hsien Hsing-an fu, and Wu-shan to Ichang, on the Yangtse.)

April 12 to May 10. On foot, with coolie transport, across the Ch'in ling Mountains, with topographical survey and geological reconnoissance from Chou-Chih hsien, in the Wei Valley, to Shih-ch'uan hsien, in the Han Valley; geology of the pre-Cambrian and metamorphic Paleozoic; physiography of the mountains of southern Shensi.

May 11 to 14. At Shih-ch'uan hsien 1 day and boat trip with route traverse down the Han River 3 days to Hsing-an fu.

May 15 and 16. At Hsing-an fu, engaging coolies.

May 17 to June 6. Crossing the mountains via Ping-li and Ta-ning hsien to Wu-shan on foot with coolie train; topographical survey, geological reconnoissance of Paleozoic strata from Cambrian to Upper Carboniferous, with studies of the physiography.

June 6 to 8. By boat on the Yangtse River, Wu-shan to Ichang, with geological notes; discovery of glacial deposits of early Cambrian age.

June 9 to 13. En route, by steamer on the Yangtse River, Ichang to Shanghai.

June 14 to 20. At Shanghai.

June 20 to July 15. En route, by steamer *Mongolia*, from Shanghai to San Francisco.

## RESULTS OF RESEARCH.

### CONTRIBUTIONS TO GEOLOGY OF THE PALEOZOIC ERA.

*Cambrian Strata and Faunas.*—In Shantung the succession of strata of Cambrian age was established by definite measurements in connection with topographical surveys, affording a complete record. The work was carried out in two districts, seventy miles apart, and variations of strata were thus determined from place to place. The results are embodied in detailed geological maps on a large scale. The observed facts show that the physical history of Shantung was closely parallel in character to that of the Central Appalachian province of North America, there being in each region a basal unconformity with very ancient metamorphic rocks, a sequence of clayey and limey deposits several thousand feet thick, and a predominance of limestone in the upper part of each series.

From these Cambrian strata of Shantung collections of fossils were secured which thoroughly represent the faunas of the province. They comprise many forms found in North America and exhibit the succession of genera typical of the Lower, Middle, and Upper Cambrian. Olenellus, the widespread genus universally found as the forerunner of the varied life of the early Paleozoic, here occupies its usual position near the base of the section. More complete collections and more perfect specimens may be secured in some future work covering the whole province in detail, but such operations were beyond the scope of this expedition, except at the cost of abandoning other exploration.

In the province of Liaotung, southern Manchuria, according to Baron von Richthofen, there are strata older than the Cambrian, and it was thought possible that we might there find a pre-Cambrian fauna. The plan of travel in China first included a survey of Liaotung for this purpose, but in consequence of the sensitive political conditions in Manchuria detailed operations were given up, on the advice of the American minister at Pekin. Nevertheless, Mr.

Blackwelder, in December, carried out instructions for a reconnoissance to cover the point. The Yung-ning sandstone described by Baron von Richthofen was found, but contains no fossils and is stratigraphically probably not older than the lowest Cambrian of Shantung.

In Chihli and Shansi, north of latitude 38°, Cambrian strata very similar to those in Shantung were discovered. Their general relations and sequence were observed and their occurrence noted in connection with topographic surveys, so that the areas can be represented in a general way on the maps. Fossils were secured which suffice to identify the Lower, Middle, and Upper Cambrian terranes.

In southern Shensi, about latitude 31° 30′, occur limestone strata several thousand feet thick, which are but sparsely fossiliferous. In river pebbles derived from them fragments of Olenellus were found, which proved that the older strata are Lower Cambrian. Unfortunately the beds did not appear along the route traveled, but the limestones so closely resemble those of Middle and Upper Cambrian age of Shantung and Shansi in certain unusual lithologic characters as to leave little doubt of their being part of a corresponding sequence, at the base of which the Olenellus occurs.

*Cambrian Glacial Deposits.*—On the Yangtse River, in latitude 31°—*i. e.*, as far south as New Orleans, not high above sea-level—a large body of glacial till was discovered. It is unstratified, a mass of indurated clay and heterogeneous bowlders, many of which exhibit glacial polish and striæ. Specimens submitted to Professor Chamberlin and other expert glacialists are pronounced by them unquestionably of glacial origin. This deposit lies near the base of the Paleozoic system, beneath limestone which in its lowest layers contains pebbles from the till, and which is in all probability of Lower Cambrian age, as the specimens of Olenellus referred to in the preceding paragraph were found in this district. The body of till is 170 feet thick, a very considerable mass. It demonstrates the existence of glacial conditions in a very low latitude in the early Paleozoic. A similar occurrence at a closely related Cambrian epoch has been reported from Scandinavia, but nowhere else has like evidence been found. This discovery takes a place among the unique facts of geological history, and the latitude, the conditions of occurrence, and the conclusiveness of the evidence being considered, it will have great weight in reference to theories of climatic change.

*Ordovician Strata.*—Throughout Shantung, Chihli, Shansi, and Shensi, between latitude 30° and 40°, there is a limestone 3,000 feet or more thick, which is stratigraphically continuous with the

Upper Cambrian. It contains few fossils, but enough were found to demonstrate its Ordovician age. It is the counterpart of the Shenandoah limestone of Virginia in lithologic character and in its relation to the Cambrian series, and it thus appears that in eastern Asia, as in eastern America, the passage from the Cambrian to the Ordovician was without break in the sequence of strata or associated faunas. This relation was not recognized by Baron von Richthofen, who mistook the Ordovician limestone for the Carboniferous, which it closely resembles.

In southern Shensi, on the Ta-ning River, fossils of Cincinnatian age were collected in abundance at a single locality, the only one in which the terrane was seen. No previous record of the occurrence of strata of this age in China is known to me.

*Carboniferous Strata.*—The contribution to knowledge in reference to the Carboniferous is a correction of former views. In southern China there is a conspicuous Carboniferous limestone; in southern and northern China there is a similar limestone of Ordovician age. Baron von Richthofen, not recognizing the distinction, mapped the Ordovician of northern China as Carboniferous, but it is now possible to indicate correctly the limits within which the Carboniferous occurs, so far as our observations go.

Connected with the preceding is the recognition of an extensive unconformity between the Ordovician and the Coal Measures throughout northern China.

### CONTRIBUTIONS TO GEOLOGY OF THE PRE-CAMBRIAN.

*Basement Complex.*—The occurrence in Asia of ancient crystalline schists and intrusive igneous rock, constituting a basement complex beneath the obviously stratified series, has long been known through the work of Baron von Richthofen and of Russian explorers. The present contribution to knowledge of the system consists of more detailed observations of the relations of its members—several kinds of schists, gneiss, granite, and basic intrusives. One area, the Tai-shan in Shantung, was somewhat closely studied, and from it as well as from widely separated localities in Chihli, Shansi, and Shensi, specimens were secured for petrographic investigation.

*Pre-Cambrian Sedimentary Series.*—The Wutai-shan and adjacent mountains in northern Shansi consist of rocks ranging in age from the extremely ancient basement complex to the Coal Measures. Between the complex and the base of the Cambrian are two series, both of which were described by Baron von Richthofen, the older as the "Wutai schist," the younger as the "Lower Sinian." In

surveying the mountains the relations of these two series to earlier and later ones have been more exactly determined, and the constituent members of each series have been noted. Two points of interest may be stated: a conglomerate in the Wutai schist contains pebbles of quartzite derived from an older sedimentary formation which has not been surely identified in place; and with reference to the Lower Sinian, its relation to the Upper Sinian (Cambrian) was observed along an extensive contact and found to be that of marked unconformity. The Lower Sinian therefore falls out of the Cambrian and takes a position in the geologic column parallel with that of the Belt terrane of the northwestern United States. In the Belt terrane, after prolonged search, Mr. Walcott discovered certain fossils, the oldest definite forms known. The Lower Sinian was examined by us for fossils without success, but the strata are of limestone and shale favorable to the preservation of organic remains, and, considering the rare occurrence of the earliest fossils, the negative result of this preliminary survey should not be considered final. The Lower Sinian may repay exhaustive study by results of which only exceptional localities offer any prospect anywhere in the world.

### CONTRIBUTIONS TO THE HISTORY OF MOUNTAINS.

*The Old View and the New.*—When Baron von Richthofen made his observations in China the view prevailed that mountains were fixed features of the earth's surface, which dated in any particular case from a geologic age, however remote, represented by the youngest rocks in the mountain structure. This view is expressed in all the accounts of Asia by European scientists. In America, during the last fifteen years, through the study of topographic forms, it has been shown that the mountains of this continent are relatively recent features as compared with the rocks composing them, and owe their elevation to forces acting during the latest geologic periods down to the present. It was a point of prime interest in the comparative geology of continents whether the American methods of study applied to Asia would show that mountain growth had recently been active there also. The observations of this expedition demonstrate clearly that the histories of mountains in North America and China run closely parallel in time, in manner of development, and in resulting features of relief. The studies of Professor Davis in western Asia point in the same direction, and the (as yet unpublished) investigations of Professors Penck and De Martonne in the Alps and Karpathians extend the generalization to central Europe.

The conclusion that mountains are recent growths—indeed, are in some districts now actively growing—is far-reaching in effect on theories of the earth's internal energy and its manifestations.

*Mountain Growths of China.*—The oldest topographic surface recognized in China was once in part a hilly region, in part a nearly level peneplain, which stood but slightly above sea level during early and perhaps middle Tertiary time. That surface has since been warped. Where depressed it lies below sea level, buried under the alluvial deposits of the Huang-ho and the Yangtse-kiang; where elevated it tops the summits of mountain masses, even the Wutai-shan, at an elevation of 10,000 feet. Where the plain has been elevated, valleys and canyons are sculptured in the subjacent rock masses, and these, in their relative positions and in their forms, express the conditions under which they have been modeled. These conditions have varied from epoch to epoch, and the history of the changes is read in the mountain forms. Through our observations in China we recognize that the surface has been warped intermittently, episodes of relatively active movement having alternated with those of comparative quiescence. These variations distinguish stages of development which are capable of arrangement in a general sequence parallel with the history of mountains in North America.

A discussion of the events of mountain growth is beyond the scope of this preliminary report, but it is of interest to note that the great ranges in eastern Asia, like those of western North America, are of very recent development. Conspicuous among the mountains we have seen are the Ho-shan of Shansi and the Hua-shan of Shensi. They are ranges of great altitude, with bold, even fronts like the Wasatch in Utah, and, like the Wasatch, they each define one margin of a dislocation in the earth's superficial crust, along which displacement has very recently occurred or is now going on. The superb scenery of the great gorges of the Yangtse and of the mountain region which extends north to the Huang-ho is a result of very recent unwarping, in spite of which the larger streams have held their courses, as the Columbia River has across the rising Cascade range.

In addition to the interest which attaches to the history of mountain growth in China for itself, and to the broad inferences which may follow from a comparison with the mountains of North America, the study affords important criteria for the new science of physiography, since the conditions in China have in some respects been peculiar. Among the interesting problems upon which our physiographic investigations throw light is that of the loess.

*The Loess.*—Among the problems of geology in China, none has been more widely discussed than that of the origin and occurrence of the extensive and thick mantle of yellow earth, characterized by fine texture and vertical structure, to which Baron von Richthofen gave the name of *loess*. It is typically developed in Shansi and northern Shensi, along the route of our expedition, and presents those anomalie sof distribution which in part led Von Richthofen to the theory that the material is wind-carried dust from the deserts of central Asia. It occurs frequently on mountain slopes, where waters could not possibly have deposited it under existing topographic conditions, and he consequently rejected the idea that it might have been deposited by rivers. The hypothesis of purely eolian origin excites doubt in view of the enormous mass of the loess, as well as because of facts of occurrence, and in America leading students of the loess of the Missouri Valley incline to attribute its special character to interaction of rivers and winds. The problem in China is much more complex than in the United States, but light is thrown on it by a proper understanding of the history of the mountains.

In the development of the topography there was a stage when there were broad valleys which became overspread with alluvial deposits. Later the surface was strongly warped, stream courses became readjusted, and the alluvium was in part redistributed, but in part remains in remnants on hills produced by the warping of the flood-plains. The alluvium was and is loess. It owes its fine, uniform texture to sifting by wind, and its peculiar structure may be a physical effect of the capillary movement of water in the impalpable dust beds, but it probably accumulated as the alluvium of the Huang-ho and other streams. The interaction of winds and rivers was specially favored by the climatic conditions of the Pleistocene, which in America and Europe gave rise to the great ice-sheets. In China there was no glaciation, but, according to the history of the mountains, the time of loess accumulation fell in early Pleistocene, and the deposit may be considered the representative in eastern Asia of our glacial beds. The observations made during our journey bring the facts of the loess into accord with the physiographic history of the region in which it occurs, and into agreement with the similar formation in America. We supplement the views of Baron von Richthofen: We regard the agency of wind, upon which he laid stress, as of prime importance in producing the peculiar texture of the loess and in distributing the dust locally; we show that, at present, winds and streams are both engaged in transporting it,

streams doing the greater part of the work; that during an epoch shortly preceding the present, topographic conditions were unlike those now existing, and were favorable to the accumulation of alluvium in places where its situation is now peculiar, and we reason that the same agencies, streams and winds interacting, spread the deposit initially.

### ON THE INFLUENCE OF MAN.

*Denudation and Terracing.*—Northern China is remarkably bare of trees, shrubs, or herbage, except shade-trees, fruit-trees, crops, and on steep hillsides strong-rooted, ineradicable grass. This condition is the work of man. Unchecked by public opinion or by regard for future generations, the Chinese have destroyed vegetation in supplying individual need. The process still continues, as, pressed by the necessity for fuel, they scratch up the scanty grass by the roots with a specially contrived tool.

The effects of denudation are more pronounced in Shantung than in any other province visited. For 3,000 years or more the process has been efficiently promoted by a dense population, which has removed not only vegetation, but also soil, wherever the latter was not deep enough to grow crops or did not present a nearly level surface. The disastrous effects of heavy rains must early have led to the practice of terracing, which is now universal and which is extended to the utmost limits of gathering soil. In a few rare instances in Shantung we found thin soil on steep slopes near mountain tops, dug up above a stone wall with the obvious purpose that it should be washed down and caught. Everywhere below was the perfected system of terraces—soil reservoirs.

Northern Shansi presents similar conditions, but in a less advanced stage. The Wutai-shan affords an especially interesting example. The mountains were forest-covered with pines up to the time of the Emperor Chien Lung, who, about 160 years ago, issued an edict that the district which had previously been inhabited only by priests should be populated. The trees were rapidly destroyed; the great bare mountains are now at the mercy of the elements, and huge gullies, eating their way toward the summits, tell what progress denudation is making, as do also the wastes of gravel and sand along the streams. The method of terracing, in general use elsewhere, is but rudely developed in the Wutai, yet a beginning is made, and the necessities of agriculture will demand that it be perfected.

In southern Shensi the climate is less rigorous and vegetation is more luxuriant. There forests of pine still cover large areas in dis-

tricts so remote or so difficult of access that the Chinese can not utilize the timber, and slopes which are not in cultivation become overgrown with shrubs.

These observations of destructive and constructive activity in different stages of progress afford important suggestions for the people of less densely populated countries. The photographs secured strikingly illustrate the facts noted.

## CONTRIBUTIONS TO GEOGRAPHY.

*Mother-maps of China.*—The original surveys upon which maps of China are based are of four classes: Chinese maps; the astronomical positions determined by the Jesuits prior to 1730; travelers' route traverses; and modern surveys by military intelligence branches of the English, German, French, and Russian governments. Travelers' traverses, executed with compass, barometer, and rude methods of measuring distances, have supplied the greater part of cartographic data; and the contribution of Baron von Richthofen is conspicuous in all maps of districts through which he journeyed. The recent surveys by military officers in north China have not been extended beyond the great plain, except perhaps along certain principal highways, and in Shantung, where the Germans are making a detailed map. The geographic surveys of the Carnegie expedition are contributions which, for accuracy of positions, rank with the best of the military surveys, and in topographic expression excel them.

*Methods of Topographic Survey.*—In general the methods of the survey were those developed with the plane-table in the western United States by the United States Geological Survey, except that primary triangulation points were not available. When practicable a base line was measured, triangulation was expanded from it by theodolite or plane-table, and stations were occupied with the plane-table for location of numerous points by intersection and for topographic sketching. Where this graphic triangulation was not practicable a stadia traverse was carefully run. Relative altitudes were determined by vertical angles. At Pao-ting fu the elevation of the Belgian railway was taken as a datum, and heights above sea-level were thus closely determined. Elsewhere a datum based on aneroid barometers was the best available, and absolute elevations are merely approximate, although relative heights are as nearly correctly determined as the vertical-angle method permits.

Throughout Mr. Sargent's work determinations of latitude were made both by sextant and by observations on Polaris whenever con-

ditions permitted. Polaris was also frequently observed for azimuth. The surveys in Shantung, which I executed before Mr. Sargent joined the party, lacked these checks, but were otherwise carried out with plane-table and graphic triangulation by the methods which he employed.

*Summary of Geographic Results.*—All our topographic maps are on a scale of 1 : 90,000, with contour interval of 100 feet.

In Shantung: Topographic maps of two special areas, by Bailey Willis. The Ch'ang-hsia district, 135 square miles; the Hsin-tai district, 230 square miles. These are base maps to illustrate geological relations and physiographic types. They are too limited to have much geographic significance. In exchange for data relating to names, on request of Oberlieutenant Kleeman, in charge, copies were furnished the German Intelligence Department at Tientsin, to be incorporated in the map of Shantung.

In Chihli and Shansi: Topographic maps of the route from Pao-t'ing fu via Wutai-shan to Tai-yuan fu, by R. H. Sargent. The area represented is 250 miles long by 5 to 20 miles wide. The survey was executed by graphic triangulation expanded from a base line at T'ang hsien, Chihli, to a second base line at Tai-yuan fu, Shansi. A short stretch on the plain from Pao-t'ing fu to T'ang h'sien was measured by stadia traverse, triangulation being impracticable. The latitude and longitude of Pao-t'ing fu were accepted as determined by the British Intelligence Branch, and all other points of the survey, including Wutai-shan and Tai-yuan fu, are fixed by the triangulation with reference to this datum. Independent observations for latitude serve as checks; by means of contours the elevation and form of topographic features are expressed in a manner adequate for engineering plans or physiographic studies, as they have not previously been for any considerable area of China of which maps are published. As the survey extends from the low plain at Pao-t'ing fu over the mountain passes into Shansi covers much of the Wutai-shan at altitudes of 8,000 to 10,000 feet, and descends into the basin of Tai-yuan fu, along an old but not previously surveyed main route of commerce, it is an original contribution of much value.

In Shensi: A continuous traverse, about 375 miles long, in three sections. First section: From Chou-chih hsien, in the Wei Valley, across the Ch'in-ling Mountains, to Shih ch'uan-hsien, on the Han River, 100 miles; expanded from a base line by graphic triangulation to the crest of the Ch'in-ling range and thence extended as a stadia traverse, with topographic sketching, the conditions being

unfavorable for triangulating. Second section: From Shih-ch'uan hsien to Hsing-an fu by boat, 100 miles; directions by compass and distances by time, with estimate of rate of progress. Third section: From Hsing-an fu, on the Han, to Wu-shan, on the Yangtse River, 175 miles; by stadia traverse, with topographic sketching. The whole was checked throughout by observations for latitude. These three sections furnish a map along routes not previously surveyed, except that the third section coincided with a military reconnoissance executed a few weeks earlier by the British party under Colonel Manifold, of whose plans we were not aware in selecting a way across the mountains. From the Wei Valley to the Yangtse is a mountainous region, scarcely known as to general features and not at all as to details which express physiographic history or the difficulties opposed to engineering; the Han is an important route of commercial exchange of which our traverse covers a stretch hitherto not mapped; the contribution to geographic knowledge is one of definite facts where previous information was vague or lacking.

### CONTRIBUTIONS TO ZOÖLOGY.

*Mammals, Birds, and Reptiles.*—Through the special knowledge of natural history possessed by Mr. Blackwelder, and especially of birds, the scientific results secured by the expedition are enriched by his observations on the distribution and habits of mammals, birds, and reptiles. His notes cover, of mammals, 11 species, 1 specimen; of birds, 150 species, 64 specimens; of reptiles and amphibians, 10 species, 10 specimens. Those species not represented by specimens are described from notes taken in the field. Among the data is a daily roll-call of the birds seen from October to June in the widely separated districts of plain and mountain which we traversed.

### ARTESIAN WATERS.

*Peking and Vicinity.*—At the request of the American minister, Hon. E. H. Conger, and by the authority of the Director of the Carnegie Institution, I made an investigation of the artesian water conditions in the vicinity of Peking, and reported favorably. The report was addressed to Mr. Conger, was by him forwarded to the State Department, and thence referred to the United States Geological Survey for suggestions in regard to well boring.

The water-supply of Pekin is very inadequate and seriously contaminated. Should a successful well be drilled it will lead to material benefit to the American Legation, to the foreign community, and in time possibly to the Chinese population.

## PHOTOGRAPHS.

*Snap Shots.*—The party was equipped with pocket kodaks and with one No. 4 Panoram kodak, with which altogether a thousand or more snap shots were secured. These are useful as notes and 5 to 10 per cent of them may be appropriate in illustrating reports.

*Time Photographs.*—Being supplied with a 6½ by 8½ camera and Zeiss combination lens, loaned by the United States Geological Survey, I was able to take many photographs not within the reach of snap-shot cameras. About 250 good negatives were obtained, comprising subjects in scenery, architecture, temples, idols, and portraits of much interest.

# INDEX.